MISCELLANEOUS WORKS

OF THE LATE

THOMAS YOUNG, M.D., F.R.S., &c.,

AND ONE OF THE EIGHT FOREIGN ASSOCIATES OF THE NATIONAL INSTITUTE OF FRANCE.

VOLS. I. & II.,

INCLUDING HIS SCIENTIFIC MEMOIRS, &c.,

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ERRATA.

Page 11, line 14 from top, *for* "$\frac{\frac{1}{2}n}{\frac{1}{2}n+1}+\frac{1}{1+\frac{2}{n}}$" *read* "$\frac{\frac{1}{2}n}{\frac{1}{2}n+1}=\frac{1}{1+\frac{2}{n}}$"

" 17, " 9 " top, *for* "$\left(-\frac{1}{n}\right)^n$" *read* "$\left(1-\frac{1}{n}\right)^n$"

" 21, " 1 " top, *for* "$-\mathrm{d}x\frac{fyxx\mathrm{d}x}{xx}$" *read* "$=\mathrm{d}x\frac{fyxx\mathrm{d}x}{xx}$"

" 24, " 13 " top, *for* "$(\cos.{}^2x-b)^2$" *read* "$(\cos. x-b)^2$."

" 30, " 17 " top, *for* "$=u$" *read* "$u=$."

" 37, " 15 " top, *for* "at" *read* "or."

" 99, " 3 " bottom, *for* "$\frac{n}{zz'}$" *read* "$\frac{n^3}{z^2}$"

No. XXVII.

APPENDIX TO CAPTAIN KATER'S ACCOUNT OF EXPERIMENTS FOR DETERMINING

THE LENGTH OF THE PENDULUM

VIBRATING SECONDS IN THE LATITUDE OF LONDON.*

From the Philosophical Transactions for 1818, p. 95.

My dear Sir,

I cannot forbear to congratulate you on the discovery of the singular property of your pendulum, which has lately been demonstrated by Mr. Laplace, since it appears to remove the only doubt, that could reasonably be entertained, of the extreme accuracy of the results of your experiments. The correction for the curvature of the rolling surfaces, in the case of a simple pendulum, is very easily obtained from the geometrical determination of the curve described, although Mr. Laplace's train of reasoning, from mechanical principles, is somewhat too elaborate to be readily followed through all the symbols in which it is enveloped: and the same geometrical considerations appear, at first sight, to be equally applicable to the case of compound pendulums in general, since the

* The reciprocity of the centres of suspension and oscillation, a theorem demonstrated by Huygens, was made use of by Capt. Kater for determining very accurately the length of the seconds pendulum; and the details of the processes and experiments which were made use of for that purpose, form the subject of the well-known paper to which the article in the text was appended. It gives a very ingenious investigation of a theorem, of no small importance in the verification of the methods which were employed in this determination, which was first demonstrated by Laplace in the 'Connaissance du Tems.' Another and a much more simple investigation was pointed out at the end of a paper (No. XXVIII.) which immediately follows the one given in the text. The theorem is now found in Dr. Whewell's 'Dynamics' and in other elementary books. See also Sir J. Lubbock's 'Memoir on the Pendulum' in the Philosophical Transactions for 1830, where the problem is discussed in its most general form.—*Note by the Editor.*

motions of all their effective parts are concentric with those of a simple one similarly suspended. But upon further reflection, it becomes evident that these motions, though concentric, are related to each other in proportions somewhat different from those of a similar pendulum vibrating on a single point, and it is therefore necessary to determine the modification of the motion produced by this difference of connexion. The investigation may however be conducted in a method much more simple and intelligible to ordinary capacities, than that which has been adopted by the celebrated mathematician to whom we are indebted for the theorem ; and I am tempted to send you an "*aperçu*" of the reasoning by which I have satisfied myself respecting it.

It follows immediately from the general theorem for finding the curvature of trochoids of all kinds (Lectures on Nat. Phil. II. p. 559), that the radius of curvature of the path of any point, in the rod of a pendulum supported by a cylindrical axis, will initially be a third proportional to the distances of the point from the centre of the cylinder, and from the surface on which it rolls: so that when the cylinder is small and the pendulum simple, the centre of curvature of its path may be considered as situated at the distance of the radius r below the point of contact: and this is obviously the only correction required for such a pendulum as that of Borda. But when the weight is divided, or of considerable magnitude, it becomes necessary to calculate the effect of the different curvatures of the paths of its different parts, and to compare these paths with that of a pendulum A of any given length a. Supposing, for the sake of simplicity, the weight of each horizontal section to be concentrated in the vertical line, and calling the distance of any particle P below the surface of the cylinder x, the radius of curvature of its path will be a third proportional to $x + r$ and x, that is, $\frac{xx}{x+r}$; and the inclination of the curve at a given distance from the vertical line being always directly as the curvature, or inversely as its radius, the force derived from the weight of P will be to the force at an equal distance in the path of A, as a to $\frac{xx}{x+r}$, or as $\frac{a(x+r)}{xx}$ to 1. Now the point of

the rolling pendulum confined to the vertical line is not the centre of curvature, but initially the surface of the cylinder: so that this must be considered as the point of intersection with the vertical line, and as the fulcrum of the lever; consequently the distance of P from the vertical line will be, to that of the pendulum A, as x to a, and its immediate force will be $\frac{a(x+r)}{xx} \cdot \frac{x}{a} \cdot P = \frac{x+r}{x} P$; but this force, acting only at the end of a lever x, will have its effect at A again reduced in the ratio of x to a, and will then become $\frac{x+r}{a} P$; and if we express the sum of all the similar forces belonging to the body by the character Σ, whether found by a fluxional calculation or otherwise, we have the whole force, at A, $\Sigma \frac{x+r}{a} P$. The reduced or rotatory inertia of the body, sometimes very improperly called the "momentum" of inertia, will also be expressed by $\Sigma \frac{xx}{aa} P$, being reduced in the ratio of the squares of the distances from the fulcrum; consequently the accelerative force will be to that of the pendulum A as $\frac{\Sigma \frac{xx}{aa} P}{\Sigma \frac{x+r}{a} P}$ to 1, or as $\frac{\Sigma xx P}{a \Sigma (x+r) P}$ to 1; since it is indifferent whether the integral or the differential be divided by the constant quantity a: and in order to express the length of the equivalent pendulum, we must suppose a to be as much lengthened as the force is weakened, so that we have for this length $\frac{\Sigma xx P}{\Sigma (x+r) P}$. It is obvious that the denominator of this fraction is the same that would express the force of the body with regard to the centre of the cylinder as a fixed point; and it might indeed have been inferred at once, from the principle of virtual velocities, that the force must be the same in either case, however irregular the form of the body may be: but it is somewhat more satisfactory to follow the mechanical steps by which the operation of the law takes place. If we make $r = 0$, we have $\frac{\Sigma xx P}{\Sigma x P} = l$, for the length of the equivalent pendulum when the surface of the cylinder is supposed to be the centre of suspension;

and it follows from the well known properties of the centre of gravity, that $\Sigma\, x\, \mathrm{P}$, the sum of the product of all the particles into their distances, is equal to Qd, the product of the whole weight Q into the distance of the centre of gravity from the point of suspension; and $\Sigma\, x^2\, \mathrm{P} = \Sigma\, x\, \mathrm{P}\, l = d\, \mathrm{Q}\, l$, so that the equivalent length for the rolling pendulum becomes

$$\frac{d\,l\,\mathrm{Q}}{\Sigma(x+r)\,\mathrm{P}} = \frac{d\,l\,\mathrm{Q}}{\Sigma\,x\,\mathrm{P} + \Sigma\,r\,\mathrm{P}} = \frac{d\,l\,\mathrm{Q}}{d\,\mathrm{Q} + r\,\mathrm{Q}} = \frac{l}{1+\frac{r}{d}} = l\left(1-\frac{r}{d}\right),$$

r being supposed very small; which, for a simple pendulum, when $d = l$, becomes $l - r$, as it ought to do. We must however find the displacement of the centre of suspension which is capable of producing an equal alteration in the length of the equivalent pendulum; and for this purpose we must have recourse to the theorem of Huygens, which may be easily deduced from the expression $\frac{\Sigma\,x\,x\,\mathrm{P}}{d\,\mathrm{Q}}$: for calling $x - d$, the distance of any particle of the body from its centre of gravity, y, we have $x^2 = (d+y)^2 = d^2 + 2dy + y^2$, and $\Sigma x^2\mathrm{P} = \Sigma d^2\mathrm{P} + 2d\Sigma y\mathrm{P} + \Sigma y^2\mathrm{P} = d^2\mathrm{Q} + 0 + \Sigma y^2\mathrm{P}$, the integral of $\Sigma y\mathrm{P}$, the product of the distance of each particle into its distance from the common centre of gravity always vanishing: consequently $l = \frac{\Sigma\,yy\,\mathrm{P} + d^2\mathrm{Q}}{d\mathrm{Q}} = \frac{\Sigma\,yy\,\mathrm{P}}{d\mathrm{Q}} + d$, and $l - d = \frac{\Sigma\,yy\,\mathrm{P}}{d\mathrm{Q}}$; which is Huygens' theorem: the constant quantity $\frac{\Sigma\,yy\,\mathrm{P}}{\mathrm{Q}}$ being equal to $dl - d^2$. If now we suppose d to be increased by the small quantity s, the reciprocal, instead of $l - d$, will become $\frac{dl - dd}{d+s} = \frac{l-d}{1+\frac{s}{d}} = (l-d)\left(1-\frac{s}{d}\right) = l - d - l\frac{s}{d} + s$, to which adding $d + s$, we have $l - l\frac{s}{d} + 2s$, the increase of the length being $\frac{2d-l}{d}s$; and making this equal to $-\frac{l}{d}r$, we have $s = \frac{-lr}{2d-l}$: and when the pendulum is inverted, substituting $l - d$ for d, the expression becomes $\frac{-lr}{2l-2d-l} = \frac{lr}{2d-l}$, which, added to the former negative value of the same quantity, must always destroy it: so that the length of the equivalent pendulum will be truly measured by the simple

distance of the surfaces of the cylinders, as Mr. Laplace has demonstrated.

There is however another correction, of which it becomes necessary to determine the value, when a very sharp edge is used for the axis of motion, as in the pendulum which you have employed: since it appears very possible, that in this case the temporary compression of the edge may produce a sensible elongation of the pendulum. But it will be found, by calculating the magnitude of this change, that when the edge is not extremely short, and when its bearing is perfectly equable, this correction may be safely neglected.

Supposing a to be the distance from the edge, in the plane bisecting its angle, at which the thickness is such, that the weight of the modulus of elasticity corresponding to the section shall become equal to the weight of the pendulum, the elasticity at any other distance x from the edge will be measured by x, while the weight is represented by a; so that the elementary increment x' will be reduced by the pressure of the weight to $\frac{x}{a+x}x'$, and the element of the compression will be $\frac{a}{a+x}x'$, and its fluxion $\frac{a}{a+x}\,dx$, of which the fluent is a H L $\frac{a+x}{a}$. Now the height of the modulus of elasticity of steel is ten million feet (Lect. Nat. Phil. II. p. 509), and the weight of a bar, an inch square, and of this height, would be about 30 millions of pounds; so that if the weight be 10 pounds, and the line of bearing an inch long, the thickness at the distance a must be one three millionth of an inch; and supposing the angle a right one, a must be $\frac{1}{4244000}$; and making $x=1$, we have the whole compression of the edge within the depth of an inch $\frac{1}{4244000}$ H L 4244001; and this logarithm being 15.26, the correction becomes equal to the 360 thousandth of an inch. If the bearing were one-tenth of an inch only, the compression for both the opposite edges would become $\frac{1}{18000}$, supposing that they retained their elasticity, and underwent no permanent alteration of form. In fact, however, the edge must be considered as a portion of a minute cylinder, which will be still less compressible than an angle contained by planes; and the happy

property, demonstrated by Mr. Laplace, will prevent any sensible inaccuracy from this cause, however blunt the edges may be, supposing that the steel is of uniform hardness in both.

Believe me, my dear Sir, very sincerely yours,

THOMAS YOUNG.

Welbeck Street, 5th Jan. 1818.

P.S. It is easy to show that the determination of the length of the pendulum, by means of a weight sliding on a rod or bar, which is the method that I have proposed as the most convenient for obtaining a correct standard, is equally independent of the magnitude of the cylinder employed. The reduced inertia $\Sigma x^2 P$ here consist of two portions: for the rod we may take the equivalent expression dlQ, which we may call axy, a being the weight of the bar (Q), x the distance (d) of the centre of gravity, and y the equivalent length (l): for the ball we must employ the formula $\Sigma x^2 P = \Sigma y^2 P + d^2 Q$, and call $\Sigma y^2 P$, u, and $d^2 Q$, bz^2, b being the weight of the ball, and z the distance of its centre of gravity from the point of suspension: and in the same manner the force $\Sigma (x + r) P = (d + r) Q$ must be composed of the two portions $a(x + r)$ and $b(z + r)$, so that the equivalent length becomes $\frac{axy + u + bzz}{a(x + r) + b(z + r)} = \frac{z^2 + \frac{axy + u}{b}}{z + \frac{ax + ar + br}{b}}$; which we may call $\frac{zz + v}{z + w} = t$. The experiment being then performed in four different positions of the weight, at the distances d', d'', and d''', so that the second value of z may be $z - d' = z'$, the third $z - d'' = z''$, and the fourth $z - d''' = z'''$, we must observe the times of vibration, and deduce from them the comparative lengths of the equivalent pendulum, t, $n't$, $n''t$, and $n'''t$: and hence the value of z, of v, and of t may be obtained, without determining w, and of course without employing the quantity r.

$$\text{I. } \frac{z^2 + v}{z + w} = t, \quad \frac{z'^2 + v}{z' + w} = n't, \quad \frac{z''^2 + v}{z'' + w} = n''t, \quad \frac{z'''^2 + v}{z''' + w} = n'''t.$$

II. $z + w = \frac{z^2 + v}{t}$, $z' + w = \frac{z'^2 + v}{n't}$, $z'' + w = \frac{z''^2 + v}{n''t}$,

$z''' + w = \frac{z'''^2 + v}{n'''t}$.

III. $z - z' = d'$; $z - z'' = d''$; $z - z''' = d'''$.

IV. $d' = \frac{z^2 + v}{t} - \frac{z'^2 + v}{n't}$; $d'' = \frac{2^2 + v}{t} - \frac{z''^2 + v}{n'''t}$; $d''' = \frac{z^2 + v}{t} - \frac{z'''^2 + v}{n'''t}$.

V. $t = \frac{z^2 + v}{d'} - \frac{z'^2 + v}{n'd'} = \frac{z^2 + v}{d''} - \frac{z''^2 + v}{n''d''} = \frac{z2 + v}{d'''} - \frac{z'''2 + v}{n'''d'''}$.

VI. By comparing the first of these equations successively with the second and third, and bringing the terms containing v to the same side, we obtain

$$v = \left(\frac{z^2}{d'} - \frac{z'^2}{n'd} - \frac{z^2}{d''} + \frac{z''^2}{n''d''}\right) : \left(\frac{1}{d''} - \frac{1}{n''d''} - \frac{1}{d'} + \frac{1}{n'd'}\right) =$$

$$\left(\frac{z^2}{d'} - \frac{z'^2}{n'd} - \frac{z^2}{d'''} + \frac{z'''^2}{n'''d'''}\right) : \left(\frac{1}{d'''} - \frac{1}{n'''d'''} - \frac{1}{d'} + \frac{1}{n'd'}\right).$$

This equation contains only the squares of the value of z with known coefficients; and if we substitute $z - d'$, $z - d''$, and $z - d'''$ for z', z'', and z''', respectively, we shall obtain an equation in the form $ez^2 + fz = g$, whence $z = \pm \sqrt{(g + \frac{1}{4}f^2)} - \frac{1}{2}f$.

T. Y.

No. XXVIII.

REMARKS ON THE PROBABILITIES OF ERROR IN

PHYSICAL OBSERVATIONS,

AND ON THE DENSITY OF THE EARTH,

CONSIDERED WITH REGARD TO THE REDUCTION OF EXPERIMENTS ON THE PENDULUM.

IN A LETTER TO CAPT. HENRY KATER, F.R.S.

From the Philosophical Transactions for 1819.

READ JANUARY 21, 1819.

MY DEAR SIR,

THE results of some of your late experiments on the pendulum having led me to reflect on the possible inequalities in the arrangement of gravitating matter within the earth's substance, as well as on the methods of appreciating the accuracy of a long series of observations in general, I have thought that it might be agreeable to you, to receive the conclusions which I have obtained from my investigations, in such a form as might serve either to accompany the report of your operations, or to be laid before the Royal Society as a distinct communication.

1. *On the estimation of the advantage of multiplied observations.*

It has been a favourite object of research and speculation, among the authors of the most modern refinements of mathematical analysis, to determine the laws, by which the probability of occurrences, and the accuracy of experimental results, may be reduced to a numerical form. It is indeed true, that this calculation has sometimes vainly endeavoured to substitute

arithmetic for common sense, and at other times has exhibited an inclination to employ the doctrine of chances as a sort of auxiliary in the pursuit of a political object, not otherwise so easily attainable: but we must recollect, that at least as much good sense is required in applying our mathematics to objects of a moral nature, as would be sufficient to enable us to judge of all their relations without any mathematics at all: and that a wise government and a brave people may rely with much more confidence on the permanent sources of their prosperity, than the most expert calculators have any right to repose in the most ingenious combinations of accidental causes.

It is however an important, as well as an interesting study, to inquire in what manner the apparent constancy of many general results, which are obviously subject to great and numerous causes of diversity, may best be explained: and we shall soon discover that the combination of a multitude of independent sources of error, each liable to incessant fluctuation, has a natural tendency, derived from their multiplicity and independence, to diminish the aggregate variation of their joint effect; and that this consideration is sufficient to illustrate the occurrence, for example, of almost an equal number of dead letters every year in a general post office, and many other similar circumstances, which, to an unprepared mind, seem to wear the appearance of a kind of mysterious fatality, and which have sometimes been considered, even by those who have investigated the subject with more attention, as implying something approaching more nearly to constancy in the original causes of the events, than there is any just reason for inferring from them.

This statement may be rendered more intelligible by the simple case of supposing an equal large number of black and white balls to be thrown into a box, and 100 of them to be drawn out either at once or in succession. It may then be demonstrated, as will appear hereafter, from the number of ways in which the respective numbers of each kind of balls may happen to be drawn, that there is 1 chance in 12½ that exactly 50 of each kind may be drawn, and an even chance that there will not be more than 53 of either, though it still

remains barely possible that even 100 black balls or 100 white may be drawn in succession.

From a similar consideration of the number of combinations affording a given error, it will be easy to obtain the probable error of the mean of a number of observations of any kind; beginning first with the simple supposition of the certainty of an error of constant magnitude, but equally likely to fall on either side of the truth, and then deducing from this supposition the result of the more ordinary case of the greater probability of small errors than of larger ones. This liability to a constant error may be represented, by supposing a counter to have two faces, marked 0 and 2: the mean value of an infinite number of trials will then obviously be 1, and the constant error of each trial will be 1, whether positive or negative.

Now in a combination of n trials with such a counter, if we divide the sum of the results by n, the greatest possible error of the mean thus found will be 1; and the probability of any other given error will be expressed by the number of combinations of the faces of n counters affording that error, divided by the whole number of combinations; that is, by the corresponding coefficient of the binomial $(1+1)^n$, divided by 2^n, the sum of the coefficients. The calculations therefore will stand thus:

	$n=2$	$n=3$	$n=4$	$n=6$	$n=8$
Coefficients	1 2 1	1 3 3 1	1 4 6 4 1	1 6 15 20...	1 8 28 56 70...
Numbers thrown	0 2 4	0 2 4 6	0 2 4 6 8	0 2 4 6...	0 2 4 6 8...
Differences from n	2 0 2	3 1 1 3	4 2 0 2 4	6 4 2 0...	8 6 4 2 0...
Errors of the means	1 0 1	1 $\frac{1}{3}$ $\frac{1}{3}$ 1	1 $\frac{1}{2}$ 0 $\frac{1}{2}$ 1	1 $\frac{2}{3}$ $\frac{1}{3}$ 0...	1 $\frac{3}{4}$ $\frac{1}{2}$ $\frac{1}{4}$ 0...
Sums of errors	1+0+1=2	1+1+1+1=4	1+2+0+2+1=6	1+4+5+0...	1+6+14+14+0...
Mean errors	$\frac{2}{4}=\frac{1}{2}$	$\frac{4}{8}=\frac{1}{2}$	$\frac{6}{16}=\frac{3}{8}$	$\frac{20}{64}=\frac{5}{16}$	$\frac{70}{256}=\frac{35}{128}$

It is easy to perceive that these coefficients must express the true numbers of the combinations, since they are formed by adding together the two adjacent members of the preceding series; thus when n is 3, 1 combination giving the number 0 and 3 the number 2, these two combinations being again respectively combined with 2 and 0 of a fourth counter, give $1+3=4$, for the combinations affording the number 2 in the next series; while each succeeding series must continue to begin and end with unity, since there is only one combination that can afford either of the extremes.

In order to continue the calculation with greater convenience, we must find a general expression for the middle terms, 2, 6, 20, 70..., neglecting the odd values of n. The first, 2, is made up of $(1 + 1)$, the second, 6, is $2\ (2 + 1)$; 20 is $2\ (6 + 4)$ and $70 = 2\ (20 + 15)$: or $6 = 2\,(2\,.\,\frac{3}{2})$, $20 = 2\ (6\,.\,\frac{5}{3})$, $70 = 2\ (20\,.\,\frac{7}{4})$, whence the series may easily be continued at pleasure, multiplying always the preceding term by $\frac{6}{2}$, $\frac{10}{3}$, $\frac{14}{4}$, $\frac{18}{5}$, ... We have also $6 = 16\,.\,\frac{3}{8} = 2^4\,.\,\frac{3}{8}$, $20 = 2^6\,.\,\frac{5}{16}$, and $70 = 2^8\,.\,\frac{35}{128}$: consequently the terms of this series, divided by 2^n, will always express the mean errors already calculated. From this value of the middle term we may easily deduce that of the neighbouring terms by means of the original formula $n\,.\,\frac{n-1}{2}\cdot\cdot\,\frac{\frac{1}{2}n+1}{\frac{1}{2}n}\cdot\frac{\frac{1}{2}n}{\frac{1}{2}n+1}\cdot\frac{\frac{1}{2}n-1}{\frac{1}{2}n+2}\cdot\cdot$; the first factor less than unity being always $\frac{\frac{1}{2}n}{\frac{1}{2}n+1}+\frac{1}{1+\frac{2}{n}}$. The magnitude of the mean error is exhibited in the annexed table.

n	Mean error.
2	.500000
4	.375000
6	.312500
8	.273437
10	.246094
12	.225586
14	.209473
16	.196381
18	.185471
20	.176196
30	.144466
40	.125363
50	.112271
60	.102574
70	.095022
80	.088924
90	.083868
100	.079586

The general expression for this series being $\frac{1}{2}\,.\,\frac{3}{4}\,.\,\frac{5}{6}\,...\,\frac{n-1}{n}$, it is obvious that if we multiply it by $\frac{2}{3}\,.\,\frac{4}{5}\,...\,\frac{n-2}{n-1}$, the product will be $\frac{1}{n}$, whatever the value of n may be: and when that value is large, the factors of these two expressions will approach so near to each other that they may be considered as equal; consequently the corresponding terms of either, taken between any two large values of n, will vary in the subduplicate ratio of n, since their product, which may be considered as the square of either, varies in the simple ratio of n, so that the mean error may ultimately be expressed by $\sqrt{\frac{1}{pn}}$. The value of p evidently approximates to that of the quadrant of a circle, of which the radius is unity: thus for $n = 10$ it is 1.6512, and for $n = 100$, 1.5788, instead of 1.5708; and

the ultimate identity of these magnitudes has been demonstrated by Euler and others. (See Mr. Herschel's Treatise on Series, in Lacroix, Engl. Ed., n. 410.)

The fraction thus found, multiplied by 2^n, gives the number of combinations expressed by the middle term, in which the error vanishes, when n is even: and the whole number of combinations being also 2^n, it is obvious that the fraction alone must express the probability of a result totally free from error. The neighbouring terms on each side, for $n = 100$, are .078025, .073524, and .066588, the sum of the 7 being .515860; and since this sum exceeds $\frac{1}{2}$, it is obviously more probable that the result of 100 trials will be found in some of these seven terms, than in any of the remaining 94, and that the mean error will not exceed $\frac{3}{50}$. When n is so large, that the terms concerned may be considered as nearly equal, the factors $\frac{\frac{1}{2}n}{\frac{1}{2}n+1}$, $\frac{\frac{1}{2}n-1}{\frac{1}{2}n+2}\ldots$, may be expressed by $1-\frac{2}{n}$, $1-\frac{6}{n}$, $1-\frac{10}{n}\ldots$, and the terms themselves by 1, $1-\frac{2}{n}$, $1-\frac{8}{n}$, $1-\frac{18}{n}\ldots$, the negative parts forming the series $\frac{2}{n}$ (1, 4, 9 . . .) of which the sum, for q terms, is $\frac{2}{n}(\frac{1}{3}q^3+\frac{1}{2}q^2+\frac{1}{6}q)$ or ultimately $\frac{2}{3n}q^3$; consequently if we call the middle term e, we must determine q in such a manner as to have $e\,(2q-\frac{4}{3n}q^3)=\frac{1}{2}-e$, and $q\,(1-\frac{2}{3n}q^2)=\frac{1}{4e}-\frac{1}{2}$; but e has been already found in this case, $=\sqrt{\frac{1}{pn}}$, and neglecting at first the square of q, we have $q=\frac{1}{4}\sqrt{(pn)}-\frac{1}{2}$, and $q^2=\frac{1}{16}pn$, whence $\frac{2}{3n}q^2=\frac{1}{24}p$, and $1-\frac{2}{3n}q^2=.93455$; hence, for a second approximation, $.93455\,q=\frac{1}{4e}-\frac{1}{2}$, and $q=.2674\sqrt{(pn)}-.53$; and by continuing the operation we obtain $.9235\,q=\frac{1}{4e}-\frac{1}{2}$, and $q=.271\sqrt{(pn)}-.54$; consequently the probable error, being expressed by $\frac{2q}{n}$, will be $.542\sqrt{\frac{p}{n}}-\frac{1.08}{n}=\frac{.679}{\sqrt{n}}-\frac{1.08}{n}$. This formula, for $n=100$, becomes .0571, and for $n=10000$, $.00679-.00011=.00668$.

We must not, however, lose sight, in this calculation, of the original condition of liability to a certain constant error in each trial. For example, we may infer from it, that if we made 100 observations of the place of a luminary, each differing 1′ from the truth, but indifferently on either side of it, the error of the mean result would probably not exceed $\frac{3}{50} \cdot 1' = 3.6''$; and that in 1000 observations it would probably be reduced to about a second. Now although in the methods of observing which we employ, the error is liable to considerable variations, yet it may be represented with sufficient accuracy, by the combination of two or more experiments in which the simpler law prevails. For example, the combination of two counters, such as have been considered, is equivalent to the effect of a die with four faces, or a tetraedron, marked 0, 2, 2, and 4, or with errors expressed by 1, 0, 0, and — 1; the combination of three counters is represented by a die having eight faces, or an octaedron, with the errors 1, $\frac{1}{3}$, $\frac{1}{3}$, $\frac{1}{3}$, $-\frac{1}{3}$, $\frac{1}{3}$, $-\frac{1}{3}$, -1; and the combination of four by a solid of 16 sides, with the errors 1, $4 \times \frac{1}{2}$, 6×0, $4 \times -\frac{1}{2}$, -1. These distributions evidently resemble those which are generally found to take place in the results of our experiments; and it is of the less consequence to represent them with greater accuracy, since the minute steps, by which the scale of error varies, have no sensible effect on the result, especially when the number of observations is considerable. If, for example, instead of two trials with the tetraedron, having the errors 1, 0, 0, − 1, we made two trials with a solid of 21 faces, having the errors distributed equally from 1, .9, .8.. to—1, the mean error of all the possible combinations would only vary from .375 to .349; and in a greater number of trials the errors would approach still nearer to equality.

Now in order to employ any of these suppositions for the purpose of calculation, it is only necessary to compute the corresponding mean error, and to make it equal to the actual mean error of a great number of observations. Thus, if we consider each observation as representing a binary combination of counters or constant errors, in which the mean error

is $\frac{1}{4}$, and adding together the differences of the several results from the mean, and dividing by their numbers, we find the mean error of 100 observations 1′, we must consider the original constant error as equal to 2′, which is to be made the unit for 200 primitive combinations; and $\frac{.679}{\surd 200} - \frac{1.08}{200} = .0426$; and the probable error of the mean will be $.0426 \times 120 = 5.1''$. For a quarternary combination, if the error, which amounts to $\frac{3}{8}$, be found 1′, the unit will be $\frac{8}{3}'$, and for $n = 400$, we have $.03125 \times \frac{8}{3}' = 5.0''$. And if we set out with a large number m of combinations, the mean error being $\surd \frac{p}{nm} = e$, the unit will be $e \surd (pm) - 1$, and the probable error of nm trials being equal to this unit multiplied by $.542 \surd \frac{1}{nm}$, neglecting the very small fraction $\frac{1.08}{nm}$, we have $.542 \surd \frac{p}{nm} e \surd (pm) = .542\, p \surd \frac{1}{n} e = .8514 \surd \frac{1}{n} e$: which, if e be 1′, and $n = 100$, gives again $5.1''$. It appears therefore that the supposition, respecting the number of combinations respresenting the scale of error, scarcely makes a perceptible difference in the result, after the exclusion of the constant error: and that we may safely represent the probable error of the mean result of n observations, by the expression $.85 \frac{e}{\surd n}$, e being the mean of all the actual errors.

We might obtain a conclusion nearly similar by considering the sum of the squares of the errors, amounting always to $n\, 2^{*}$: but besides the greater labour of computing the sum of the squares of the errors of any series of observations, the method, strictly speaking, is somewhat less accurate, since the amount of this sum is affected in a slight degree by any error which may remain in the mean, while the simple sum of the errors is wholly exempted from this uncertainty. In other respects the results here obtained do not materially differ from those of Legendre, Bessel, Gauss, and Laplace: but the mode of investigation appears to be more simple and intelligible.

It may therefore be inferred from these calculations, first, that the original conditions of the probability of different errors, though they materially affect the observations themselves, do

not very greatly modify the nature of the conclusions respecting the accuracy of the mean result, because their effect is comprehended in the magnitude of the mean error from which those conclusions are deduced: and secondly, that the error of the mean, on account of this limitation, is never likely to be greater than six sevenths of the mean of all the errors, divided by the square root of the number of observations. But though it is perfectly true, that the probable error of the mean is always somewhat less than the mean error divided by the square root of the number of observations, provided that no constant causes of error have existed; it is still very seldom safe to rely on the total absence of such causes; especially as our means of detecting them must be limited by the accuracy of our observations, not assisted, in all instances, by the tendency to equal errors on either side of the truth: and when we are comparing a series of observations made with any one instrument, or even by any one observer, we can place so little reliance on the absence of some constant cause of error, much greater than the probable result of the accidental causes, that it would in general be deceiving ourselves even to enter into the calculation upon the principles here explained: and it is much to be apprehended, that for want of considering this necessary condition, the results of many elegant and refined investigations, relating to the probabilities of error, may in the end be found perfectly nugatory.

These are cases in which some little assistance may be derived from the doctrine of chances with respect to matters of literature and history: but even here it would be extremely easy to pervert this application in such a manner as to make it subservient to the purpose of clothing fallacious reasoning in the garb of demonstrative evidence. Thus if we were investigating the relations of two languages to each other, with a view of determining how far they indicated a common origin from an older language, or an occasional intercourse between the two nations speaking them, it would be important to inquire, upon the supposition that the possible varieties of monosyllabic or very simple words must be limited by the extent of the alphabet to a certain number, and that these

names were to be given promiscuously to the same number of things, what would be the chance that 1, 2, 3 or more of the names would be applied to the same things in two independent instances.

Now we shall find, upon consideration, that for n names and n things, the whole number of combinations, or rather permutations of the whole nomenclature, would be $m = 1 . 2 . 3 .. n$; and that of these the number in which no one name agreed would be $a_n = m - a_1 - n . \frac{n-1}{2} . a_2 - n . \frac{n-1}{2} . \frac{n-2}{3} . a_3 . . — n . a_{n-1}$; each term expressing the number of agreements in n, $n-1$, $n-2$... instances only, and being made up of all the combinations of so many out of n things, each occurring as many times as all the remaining ones can disagree. Hence we may easily obtain the successive values of a from each other, the first being obviously 1, as a single name can only be given in one way to a single thing, therefore,

$a_1 = 1$

$a_2 = 2 - 1 = 1$

$a_3 = 6 - 1 - 3 = 2$

$a_4 = 24 - 1 - 6 - 8 = 9$

$a_5 = 120 - 1 - 10 - 20 - 45 = 44$

$a_6 = 720 - 1 - 15 - 40 - 135 - 264 = 265$

$a_7 = 5040 - 1 - 21 - 70 - 315 - 924 - 1855 = 1854$

$a_8 = 40320 - 1 - 28 - 112 - 630 - 2464 - 7420 - 14832 = 14833$

$a_9 = 362880 - 1 - 36 - 168 - 1134 - 5544 - 22260 - 66744 - 133497 = 133496$

$a_{10} = 3628800 - 1 - 45 - 240 - 1890 - 11088 - 55650 - 222480 - 667485 - 1334960 = 1334961$

From this computation it may be inferred, that, for 10 names, the probabilities will stand thus :

No coincidence	.367880	One or more	.632120
One only	.367880	Two or more	.264240
Two only	.183941	Three or more	.080300
Three only	.061309	Four or more	.018991
Four only	.015336	Five or more	.003655

Five only	.003056	Six or more	.000599
Six only	.000521	Seven or more	.000078
Seven only	.000066	Eight or more	.000012
Eight only	.000012	Nine or Ten	.0000003

The same results may be still more readily obtained from the supposition that n is a very large number; for then, the probability of a want of coincidence for a single case being $\frac{n-1}{n}$, the probability for two trials will be $\left(\frac{n-1}{n}\right)^2$, and for the whole n, $\left(\frac{n-1}{n}\right)^n = \left(1-\frac{1}{n}\right)^n$: but the hyperbolical logarithm of $1-\frac{1}{n}$ being ultimately $-\frac{1}{n}$, that of $\left(1-\frac{1}{n}\right)^n$ will be -1; consequently the probability of no coincidence will be $\frac{1}{2.718282} = .3678794$: and if n is increased by 1, each of these cases of no coincidence will afford one of a single coincidence: if by 2, each will afford one of a double coincidence, but half of them will be duplicates; and if by 3, the same number must be divided by 6, since all the combinations of three would be found six times repeated. We have therefore for

No coincidence	.3678794	One or more	$.6321206 = \frac{2}{3}-$
One only	.3678794	Two or more	$.2642412 = \frac{1}{4}+$
Two only	.1839397	Three or more	$.0803015 = \frac{1}{12}-$
Three only	.0613132	Four or more	$.0189883 = \frac{1}{53}$
Four only	.0153283	Five or more	$.0036600 = \frac{1}{273}$
Five only	.0030657	Six or more	$.0005943 = \frac{1}{1683}$
Six only	.0005109	Seven or more	$.0000834 = \frac{1}{12000}$
Seven only	.0000730	Eight or more	$.0000105 = \frac{1}{95000}$

It appears therefore that nothing whatever could be inferred with respect to the relation of two languages from the coincidence of the sense of any single word in both of them; and that the odds would only be 3 to 1 against the agreement of two words; but if three words appeared to be identical, it would be more than 10 to 1 that they must be derived in both cases from some parent language, or introduced in some other manner; six words would give near 1700 chances to 1, and 8 near 100,000; so that in these last cases the evidence would be little short of absolute certainty.

In the Biscayan, for example, or the ancient language of Spain, we find in the vocabulary accompanying the elegant essay of Baron W. von Humboldt, the words *berria*, new; *ora*, a dog; *guchi*, little; *oguia*, bread; *otsoa*, a wolf, whence the Spanish *onza;* and *zazpi*, or, as Lacroze writes it, *shashpi*, seven. Now in the ancient Egyptian, new is BERI; a dog, UHOR; little, KUDCHI; bread, OIK; a wolf, UONSH; and seven, SHASHF; and if we consider these words as sufficiently identical to admit of our calculating upon them, the chances will be more than a thousand to one, that, at some very remote period, an Egyptian colony established itself in Spain: for none of the languages of the neighbouring nations retain any traces of having been the medium through which these words have been conveyed.*

On the other hand, if we adopted the opinions of a late learned antiquary, the probability would be still incomparably greater that Ireland was originally peopled from the same mother country; since he has collected more than 100 words which are certainly Egyptian, and which he considers as bearing the same sense in Irish; but the relation which he has magnified into identity, appears in general to be that of a very faint resemblance: and this is precisely an instance of a case, in which it would be deceiving ourselves to attempt to reduce the matter to a calculation.

The mention of a single number, which is found to be indisputably correct, may sometimes afford a very strong evidence of the accuracy and veracity of a historian. If the number were indefinitely large, the probability that it could not have been suggested by accident would amount to an absolute certainty: but where it must naturally have been confined within certain moderate limits, the confirmation, though somewhat less absolute, may still be very strong. For example, if the subject were the number of persons collected together for transacting business, it would be a fair presumption that it must be between 2 or 3 and 100, and the chances must be about 100

* See the article on 'Languages' in the fifth volume of the Supplement of the Encyclopædia Britannica, p. 242, which is also reprinted in a subsequent volume of this work. (Vol. III. p. 539.)—*Note by the Editor.*

to 1 that a person reporting it truly must have some good information; especially if it were not an integral number of tens or dozens, which may be considered as a species of units. Now it happens that there is a manuscript of Diodorus Siculus, which, in describing the funerals of the Egyptians, gives 42 for the number of persons who had to sit in judgment on the merits of the deceased: and in a multitude of ancient rolls of papyrus, lately found in Egypt, it may be observed, that 42 personages are delineated, and enumerated, as the judges assisting Osiris in a similar ceremony. It is therefore perfectly fair to conclude from this undeniable coincidence, that we might venture to bet 100 to 1, that the manuscript in question is in general more accurate than the others which have been collated; that Diodorus Siculus was a well informed and faithful historian; that the graphical representations and inscriptions in question do relate to some kind of judgment; and lastly, that the hieroglyphical numbers, found in the rolls of papyrus, have been truly interpreted.

2. *On the mean density of the earth.**

It has been observed by some philosophers, that the excess of the density of the central parts of the earth, above that of the superficial parts, is so great as to render it probable that the whole was once in a state of fluidity, since this is the only condition that would enable the heaviest substances to sink towards the centre. But before we admit this inference, we ought to inquire, how great would be the effect of pressure only in augmenting the mean density, as far as we can judge of the compressibility of the substances, which are the most likely to be abundant, throughout the internal parts of the structure.

Supposing the density at the distance x from the centre to be expressed by y, the fluxion dy will be jointly proportional to the thickness of the elementary stratum, or to its fluxion

* See No. XXXIV., p. 78 of this volume. The very important principle of compressibility of a chemically homogeneous substance had not previously been noticed or its effects estimated in the theories for explaining the increase of the earth's density in passing from the surface to the centre.—*Note by the Editor.*

$-$ dx, to the actual density y, and to the attraction of the interior parts of the sphere, which varies as $\frac{\int yxx\mathrm{d}x}{xx}$; since the increment of pressure, and consequently that of density, depends on the combination of these three magnitudes: we have therefore $-n\mathrm{d}y = y\mathrm{d}x\frac{\int yx^2\mathrm{d}x}{xx}$; an equation which will readily afford us the value of y in a series of the form $1 + ax^2 + bx^4 + \ldots$.

In order to determine the coefficients, we must first find $\frac{\int yxx\mathrm{d}x}{xx} = \frac{1}{3}x + \frac{1}{5}ax^3 + \frac{1}{7}bx^5 + \ldots$, and multiplying this by $(1 + ax^2 + bx^4 + \ldots)\,\mathrm{d}x$ we obtain

$$-ny = -n - nax^2 - nbx^4 - ncx^6 - \ldots$$
$$= C + \frac{1}{2.3}x^2 + \left.\begin{matrix}\frac{1}{4.5}a \\ +\frac{1}{3.4}a\end{matrix}\right\} x^4 + \left.\begin{matrix}\frac{1}{6.7}b \\ +\frac{1}{5.6}a^2 \\ +\frac{1}{3.6}b\end{matrix}\right\} x^6 + \ldots$$

Hence, by comparing the corresponding terms, we obtain

$C =$	$-n$;		
$a =$	$-.1666667n^{-1}$	Logarithm,	9.2218487
$b =$	$.02222222n^{-2}$		8.3467875
$c =$	$-.00268960n^{-3}$		7.4296867
$d =$	$.000308154n^{-4}$		6.4887650
$e =$	$-.0000340743n^{-5}$		5.5324269
$f =$	$.00000367495n^{-6}$		4.5652514
$g =$	$-.000000389086n^{-7}$		3.5911459
[$h =$	$.00000004062n^{-8}$		2.6087]
[$i =$	$-.00000000420n^{-9}$		1.6232]
[$k =$	$-.00000000043n^{-10}$		0.6335]

After the exact determination of the first seven coefficients, the next three are obtained with sufficient accuracy by means of the successive differences of the logarithms compared with those of the natural numbers.

It happens very conveniently, that the conditions of the problem are such, as to afford a remarkable facility in deriving from this series another, which is much more convergent, and which gives us the hyperbolic logarithm of y; for since

$-n\frac{dy}{y} - dx\frac{\int yxxdx}{xx}$, and $\frac{\int yxxdx}{xx} = \frac{1}{3}x + \frac{1}{5}ax^3 + \frac{1}{7}bx^5 + \ldots$, if we multiply this by dx, and take the fluent, we shall have $\text{HL}y = -\frac{1}{n}\left(\frac{1}{2.3}x^2 + \frac{1}{4.5}ax^4 + \frac{1}{6.7}bx^6 + \ldots\right)$.

We may determine the degree of compressibility corresponding to a given value of n, by comparing the equation $-n\frac{dy}{y} = dx\frac{\int yxxdx}{xx}$, or $= dxp$, with the properties of the modulus of elasticity M, which is the height of such a column of the given substance, that the increment of density y', occasioned by the additional weight of the increment x', is always to y, as x' to M, or $\frac{y'}{y} = \frac{x'}{\text{M}}$, whence $-\frac{dy}{y} = \frac{dx}{\text{M}}$; consequently in the present case we have $\frac{dxp}{n} = \frac{dx}{\text{M}}$; and $\text{M} = \frac{n}{p}$: and if we make $x = 1$ in the value of p, we shall obtain M in terms of the radius of the earth, considered as unity. When y is invariable, and n infinite, the density being uniform, p becomes $\frac{1}{3}$, and the mean density will always be expressed by $3p$, since the attractive force is simply as the mean density: and if we divide $3p$ by y, we shall have the relation of the mean density to the superficial density. The results of this calculation, for different values of n, are arranged in the table, which will be found sufficiently accurate for the purposes of the investigation, though not always correct to the last place of figures.

n	p	$\text{M}=\frac{n}{p}$	$3p$, mean density		y	$\frac{3p}{y}$, comp. den.
∞	.33333	∞	1.0000=1 :	1.0000	1.000	1.000
1	.30290	3.301	.9087	1.1005	.855	1.065
$\frac{1}{2}$	.27735	1.803	.8320	1.2019	.738	1.127
$\frac{1}{3}$	.25535	1.305	.7660	1.3054	.646	1.185
$\frac{1}{4}$	.23688	1.055	.7106	1.4071	.575	1.24
$\frac{1}{5}$	.22058	.907	.6617	1.5111	.510	1.30
$\frac{1}{6}$	.20616	.808	.6185	1.6168	.458	1.35
$\frac{1}{7}$	.194	.736	.582	1.72	.419	1.40
$\frac{1}{8}$	.183	.681	.549	1.82	.377	1.45
$\frac{1}{9}$	.172	.646	.516	1.94	.346	1.49
[$\frac{1}{10}$	.162	.617	.486	2.05	320	1.52]
[$\frac{1}{11}$	.153	.594	.459	2.16	.298	1.55]
[$\frac{1}{12}$	.145	.575	.435	2.28	.28	1.57]
[$\frac{1}{20}$	.1	.5	.3	3.3	.17	1.8]

The reciprocals of the mean density are inserted, on account of the simplicity of the progression which they exhibit, being in the first instance precisely equal to $1 + \frac{1}{10n}$, and varying but slowly from this value.

Now if we suppose with Mr. Laplace, the mean density of the earth to be to that of the superficial parts as 1.55 to 1, it appears from this table, that the height of the modulus of elasticity must be about .594; that is, more than 12 million feet, while the modulus of the hardest and most elastic substances that have been examined, amounts only to about 10 million. It follows therefore, that the general law, of a compression proportionate to the pressure, is amply sufficient to explain the greater density of the internal parts of the earth; and the fact demonstrates, that this law, which is true for small pressures in small substances, and with regard to elastic fluids, in all circumstances, requires some little modification for solids and liquids, the resistance increasing somewhat faster than the density; for no mineral substance is sufficiently light and incompressible to afford a sphere of the magnitude of the earth, and of so small a specific gravity, without some such deviation from the general law. A sphere of water would be incomparably more dense, and one of air would exceed this in a still greater proportion: indeed even the moon, if she is really perforated, as has sometimes been believed, and contains cavities of any considerable depth, would soon have absorbed into her substance the whole of her atmosphere, supposing that she ever had one. It may be objected, that the resistance of solids to actual compression may possibly be considerably greater than appears in our experiments, since we are not absolutely certain that they do not extend in a transverse direction, when we compress them in a longitudinal one, as is obviously the case with some soft elastic substances: but this objection is removed by the experiment on the sound of ice, which affords, either accurately or very nearly, the same resistance to compression as a portion of water confined in a strong vessel; and this it could not do, if the particles of ice were allowed to expand laterally under the operation of a compressing force.

Mr. Laplace's conclusion, respecting the precise proportion of the densities, is indeed derived from another supposition respecting their variation, and would be somewhat modified by the adoption of this theory; it would not, however, be so materially altered, as by any means to invalidate the general inference. It would therefore be proper to revise the calculations derived from the lunar motions and the ellipticity of the earth, and to employ in them a variation of density somewhat resembling that which is here investigated. Indeed without reference to the effects of compressibility, it is obviously probable that the density of the earth should vary more considerably in a given depth towards the surface than near the centre, although the calculation, upon Mr. Laplace's more simple hypothesis, of a uniform variation, is much less intricate. It would, however, be justifiable, as a first approximation, to reject those terms of the series which would vanish if n and x were very small, and to make $y = 1 + ax^2$; and indeed this formula has in one respect an advantage over the series, as it seems to approach more nearly to the law of nature, in expressing a resistance somewhat greater towards the centre, where the density is most augmented: we have then, if the superficial density be to the mean as 1 to q, $q = \frac{1 + \frac{3}{5}a}{1 + a}$, whence $a = \sqrt{\frac{q - 1}{q - .6}}$; and if $q = 1.55$, $a = -.58$, affording an expression which is, in all probability, accurate enough for every astronomical purpose.

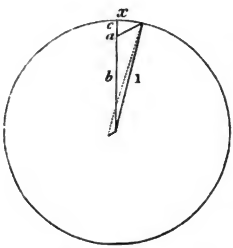
If the variation of density were supposed to proceed equably with the variation of quantity, it would obviously be as the square of the distance from the centre, and the density would be as $1 - ax^3$, the mean density being found at the surface of a sphere containing half as much as the whole earth; and this might be considered as the most natural hypothesis, if we disregarded the effects of compression: but the arithmetical progression of densities, from the centre to the surface, seems in every way improbable.

3. *On the irregularities of the earth's surface.*

A. If we suppose the plumb line to deviate from its general direction on account of the attraction of a circumscribed mass,

situated at a moderate depth below the earth's surface, the distance of the two points of greatest deviation from each other will be to the depth of the attracting point as 2 to $\surd 2$.

Let the magnitude of the additional mass be to that of the earth as a to 1, and let its distance from the centre be b; then supposing the earth a sphere, and its radius unity, and calling the angular distance of any point from the semidiameter passing through the mass x, the linear distance from the mass will be $\surd\left(\sin.^2 x + (\cos.^2 x - b)^2\right) = \surd\ (\sin.^2 x + \cos.^2 x - 2b \cos. x + b^2) = \surd\ (1 + b^2 - 2b \cos. x)$; consequently the disturbing attraction will be $\frac{a}{1 + bb - 2b \cos. x}$: but the sine of the angle subtended by the two centres of attractions will be to their distance b as sin. x to the oblique distance $\surd\ (1 + b^2 - 2b \cos. x)$; it will therefore be expressed by $\frac{b \sin. x}{\surd(1 + bb - 2b \cos. x)}$; and the sine of the very small angular deviation of the joint force from the radius will be to the line measuring the disturbing force as this last sine to the radius, the difference of the third side of the triangle from the radius being inconsiderable; consequently the deviation will be every where expressed by $\frac{ab \sin. x}{(1 + bb - 2b \cos. x)^{\frac{3}{2}}} = d$. Now in order to find where this is greatest, we must make its fluxion vanish, and $0 = \frac{\cos. x dx}{(1 + bb - 2b \cos. x)^{\frac{3}{2}}} - \frac{3}{2} \cdot \frac{\sin. x\, 2b \sin. x dx}{(1 + bb - 2b \cos. x)^{\frac{5}{2}}}$, $\cos. x\ (1 + b^2 - 2b \cos. x) = 3b \sin.^2 x$, $3b \cos.^2 x - 2b \cos.^2 x + (1 + b^2) \cos. x = 3b$, $\cos.^2 x + \frac{1 + bb}{b} \cos. x = 3$, and cos. x. $= \surd\left(3 + \left[\frac{1 + bb}{2b}\right]^2\right) - \frac{1 + bb}{2b}$; but, making $b = 1 - c$, $\frac{1 + bb}{2b}$ becomes $\frac{1 + 1 - 2c + cc}{2b} = 1 + \frac{cc}{2b}$, and c being very small, cos. x will be $\surd\left(4 + \frac{cc}{b}\right) - 1 - \frac{cc}{2b} = 2 + \frac{cc}{4b} - 1 - \frac{cc}{2b} = 1 -$



$\frac{cc}{4b}$; whence sin. $x = \surd\left(1 - \left[1 - \frac{cc}{4b}\right]^2\right) = \surd\left(1 - 1 + \frac{cc}{2b}\right) = \frac{c}{\surd(2b)}$, or simply $\surd\frac{1}{2}c$, and $c = \surd 2$ sin. x.

B. The sine of the greatest deviation of the plumb line will amount to $d = .385\frac{a}{cc}$, a being the disturbing mass, and c its depth.

Since cos. $x = 1 - \frac{cc}{4b}$, $2b$ cos. $x = 2b - \frac{cc}{2}$, and $1 + bb - 2b + \frac{cc}{2} = (1 - b)^2 + \frac{cc}{2} = c^2 + \frac{cc}{2} = \frac{3}{2}c^2$; and ab sin. x becomes $\frac{abc}{\surd(2b)}$, whence $d = \frac{abc}{\surd\left(\frac{2b.27}{8}\right)c^3} = \frac{2a\surd b}{\surd 27\,cc} = .385\frac{a\surd b}{cc}$, or simply $.385\frac{a}{cc}$; also $a = 2.618c^2d$, and $c = \surd\left(.385\frac{a}{d}\right)$. If the density were doubled throughout the extent of a sphere touching the surface internally, the radius being c, we should have $a = c^3$ and $d = .385c$, and $c = 2.6d$: but this is a much greater increase of density than is likely to exist on a large scale: so that c must probably in all cases be considerably greater than this.

C. The greatest elevation of the general surface above the sphere will be $\frac{a}{c}$, on the supposition that the mutual attraction of the elevated parts may safely be neglected.

The fluxion of the elevation is as the fluxion of the arc and as the deviation d conjointly; it will therefore be expressed by $\frac{ab \text{ sin. } x\dot{x}}{(1 + bb - 2b \cos. x)^{\frac{3}{2}}}$. Now the fluxion of $\frac{1}{\surd(1 + bb - 2b \cos. x)}$ is $-\frac{1}{2}\frac{2b \text{ sin. } x\dot{x}}{(1 + bb - 2b\cos. x)^{\frac{3}{2}}}$, consequently the fluent of the elevation will be $\frac{-a}{\surd(1 + bb - 2b\cos. x)}$: and while cos. x varies from 1 to -1, this fluent will vary from $\frac{-a}{1-b}$ to $\frac{-a}{1+b}$, the difference being $a\left(\frac{1}{1-b} - \frac{1}{1+b}\right) = a\left(\frac{1}{c} - \frac{1}{2-c}\right) = a\left(\frac{2-c-c}{2c-cc}\right)$, or simply $\frac{a}{c}$, since c is an extremely small fraction. This quantity comprehends indeed the depression on the remoter side of the sphere, which would be required to supply matter for the ele-

vation; but it is obvious that such a depression must be wholly inconsiderable.

D. The diminution of gravity to the centre at the highest point is $\frac{2a}{c}$, while the increase from the attraction of the disturbing mass is nearly $\frac{a}{cc}$, which is greater in the proportion that half the radius bears to c.

E. The increase of gravity, at the point of greatest deviation, is to the deviation itself, or its sine d, as $\sqrt{2}$ to 1.

For the deviation is the measure of the horizontal attraction of the disturbing mass, which is to its vertical attraction as sin. x to c, or as $\sqrt{\frac{1}{2}}$ to 1. Thus if d were 5″, or $\frac{5}{206265}$, the horizontal force would be $\frac{7.071}{206265} = \frac{1}{29170}$, and the acceleration of a pendulum $\frac{1}{58340}$ or 1.5″ of time in a day. It is true that a part of the deviation might depend on a defect of density as well as on an excess; but this defect could not amount to any great proportion of the whole, while the excess above the general density might easily be much more considerable, so that the acceleration of the pendulum could scarcely be *less* than a second in a day, if the greatest deviation of the plumb line were 5″: and if the deviation were 5″ at any other place, there would be a *greater* acceleration than a second at a point more or less remote from it.

F. If there were an excess of density on one side and a deficiency on the other, so as to constitute virtually two centres of attraction and repulsion, and supposing their distances to be equal, and such as to produce the greatest deviation, if the excess of density were twice as great as the deficiency, a deviation of 5″ would correspond to an acceleration of half a second; if three times as great, to $\frac{3}{4}$; if four times, to $\frac{9}{10}$; and if five, to a second.

It may perhaps be considered as an omission in this calculation, that the attraction of the parts of the earth's surface, elevated by means of the irregular gravitation, has not been

included in it. But it depends on the supposition that we may adopt respecting the cause and date of the irregularity, whether or no we ought to consider it as likely to have occasioned such a general elevation; and it does not appear that the result of the computation would very materially alter our conclusions, though it would be somewhat laborious to go through all its steps with precision. It would indeed be so much the more superfluous to insist on this minute accuracy, as variations so much more considerable in the form of the earth's surface are commonly neglected: for example, in the allowance made for the reduction of different heights to the level of the sea, which has usually been done without any consideration of the attraction of the elevated parts, interposed between the general surface and the place of observation. It is however obvious, that if we were raised on a sphere of earth a mile in diameter, its attraction would be about $\frac{1}{8000}$ of that of the whole globe, and instead of a reduction of $\frac{1}{2000}$ in the force of gravity, we should obtain only $\frac{3}{8000}$, or three-fourths as much: nor is it at all probable that the attraction of any hill a mile in height would be so little as this, even supposing its density to be only two-thirds of the mean density of the earth: that of a hemispherical hill would be more than half as much more, or in the proportion of 1.586 to 1; and it may easily be shown, that the attraction of a large tract of table-land considered as an extensive flat surface, a mile in thickness, would be three times as great as that of a sphere a mile in diameter: or about twice as great as that of such a sphere of the mean density of the earth: so that, for a place so situated, the allowance for elevation would be reduced to one-half: and in almost any country that could be chosen for the experiment, it must remain less than three-fourths of the whole correction, deduced immediately from the duplicate proportion of the distances from the earth's centre. Supposing the mean density of the earth 5.5, and that of the surface 2.5 only, the correction, for a tract of table-land, will be reduced to $1 - \frac{3}{4} \cdot \frac{2.5}{5.5} = \frac{29}{44}$, or $\frac{66}{100}$ of the whole.

4. Euler's *formula for the rolling pendulum.**

I beg leave to observe, in conclusion, with regard to Mr. Laplace's theorem for the length of the convertible pendulum rolling on equal cylinders, that its perfect accuracy may readily be inferred, without any limitation of the form of the pendulum, or of the magnitude of the cylinders, from the general and elegant investigation of Euler, which also affords us the proper correction for the arc of vibration. This admirable mathematician has demonstrated, in the sixth volume of the Nova Acta Petropolitana, for 1788, p. 145, that if we put k for the radius of gyration with respect to the centre of gravity, a for the distance of the centre of gravity from the centre of the cylinder, c for the radius of the cylinder, h^2 for $k^2 + (a-c)^2$, and b for the sine of half of any very small arc of semivibration, we shall have, for the time of a complete oscillation, $\frac{\pi h}{\sqrt{(2ag)}} + \frac{\pi bb(hh + 4ac)}{4h\sqrt{(2ag)}}$, and ultimately, if $b = 0$, $\frac{\pi h}{\sqrt{(2ag)}}$ only, which, for a simple pendulum, of the length a, k, and c both vanishing, becomes $\frac{\pi\sqrt{a}}{\sqrt{(2g)}}$, and for any other length l, $\frac{\pi\sqrt{l}}{\sqrt{(2g)}}$; consequently, making $\frac{\pi\sqrt{l}}{\sqrt{(2g)}} = \frac{\pi h}{\sqrt{(2ag)}}$, we have $\sqrt{l} = \frac{h}{\sqrt{a}}$, and $al = hh = k^2 + a^2 - 2ac + c^2$. Now if we find another value of a, which will fulfil the conditions of the equation, all the other quantities concerned remaining unaltered, and add the two values together, we shall have the distance of the centres of the two cylinders corresponding to the length l of the equivalent pendulum; but since $a^2 - (l + 2c)\,a = -k^2 - c^2$, we have $a - \frac{1}{2}l - c = \pm\sqrt{\ldots}$, and $a = \frac{1}{2}l + c \pm \sqrt{\ldots}$, so that the sum of the two values of a must be $l + 2c$, that is, the distance of the centres of the cylinders must exceed the length l by twice the radius, and l must be precisely equal to the distance of their surfaces.

Believe me, dear Sir,

Very sincerely yours,

THOMAS YOUNG.

Welbeck Street, 29th Dec., 1818.

* Supra, No. XXVII., p. 1.

No. XXIX.

A POSTSCRIPT

ON ATMOSPHERICAL REFRACTION,

ORIGINALLY APPENDED TO THE PRECEDING MEMOIR.

Reprinted, with corrections and considerable additions, from 'Brande's Quarterly Journal of Science' for 1821, vol. xi. p. 353.*

1. A SIMPLE and convenient method of calculating the precise magnitude of the atmospherical refraction, in the neighbourhood of the horizon, has generally been considered as almost unattainable; and Dr. Brinkley has even been disposed to assert the "impossibility of investigating an exact formula," that should represent all its variations, notwithstanding the "striking specimens of mathematical skill, which," as he justly observes, "have been exhibited in the inquiry." We shall find, however, that the principal difficulties may be evaded, if not overcome, by some very easy expedients.

2. The distance from the centre of the earth being represented by x, and the weight of the superincumbent column by y, the actual density may be called z, and the element of y will vary as the element of x, and as the density conjointly; consequently, $dy = -mz dx$; the constant quantity m being the

* This was the first of a series of investigations on Refraction, which appeared amongst the 'Astronomical and Nautical Collections' which Dr. Young contributed to this Journal: they led to a very acrimonious controversy between him and Mr. Ivory, to which several references will be made in the articles which follow. In the Nautical Almanac, which Dr. Young edited from 1818 to the end of his life, there is given a table of refractions calculated upon these principles, the refractions (which are nearly the same as those given by the French tables) being calculated by the formula

$$.0002825 = \frac{vr}{s} + (2.47 + .5v^2)\frac{r^2}{s^2} + 36000\frac{r^3}{s^3} + 3600\,(1.235 + .25v^2)\frac{r^4}{s^4} + \&c.,$$

where r is the refraction, v the sine, and s the cosine of the altitude.—*Note by the Editor.*

reciprocal of the modulus of elasticity. The refractive density may be called $1 + pz$, p being a very small fraction; and it is easy to see that the perpendicular u, falling on the direction of the light, will always vary inversely as the refractive density, since that perpendicular continually represents the sines of the consecutive angles, belonging to each of the concentric surfaces at which the refraction may be supposed to take place (Nat. Phil. II. p. 81); and $u = \frac{s}{1 + pz}$, s being a constant quantity. The angular refraction at each point will obviously be directly as the elementary change of this perpendicular, and inversely as the distance v from the point of incidence; whence the fluxion of the refraction will be $\frac{\mathrm{d}u}{v} = \mathrm{d}r$, as is already well known.

3. For the fluent of this expression, which cannot be directly integrated, we may obtain a converging series by means of the Taylorian theorem; but we must make the fluxion of the refraction constant, and that of the density variable; so that the equation will be $= u \frac{\mathrm{d}\upsilon}{\mathrm{d}r} . r + \frac{\mathrm{d}^2\upsilon}{\mathrm{d}r^2} . \frac{r^2}{2} + \frac{\mathrm{d}^3\upsilon}{\mathrm{d}r^3} . \frac{r^3}{2.3} + \ldots$, υ being the initial value of u, when $r = 0$. Now the whole variation, of which u is capable, while z decreases from 1 to 0, extends from $\frac{s}{1 + p}$ to s; or, since p is very small, from $s - ps$, to s; and $\mathrm{d}r$ being $= \frac{\mathrm{d}u}{v}$, we have the equation $ps = vr + \frac{\mathrm{d}v}{\mathrm{d}r} . \frac{r^2}{2} + \ldots$ But $v = \surd (x^2 - u^2)$, $\mathrm{d}v = \frac{x\mathrm{d}x - u\mathrm{d}u}{v}$, and $\frac{\mathrm{d}v}{\mathrm{d}r} = \frac{x}{v} . \frac{\mathrm{d}x}{\mathrm{d}r} - u$: and $\mathrm{d}x$ being $= -\frac{\mathrm{d}y}{mz}$, and $\mathrm{d}u = - ps\mathrm{d}z$, $\frac{\mathrm{d}x}{\mathrm{d}r} = \frac{v}{mpsz} . \frac{\mathrm{d}y}{\mathrm{d}z}$.

4. We must now determine the value of the density z, which, when the temperature is uniform, becomes simply $= y$; but for which we must find some other function of y, including the variation of temperature; and we may adopt, for this purpose, the hypothesis lately advanced by Professor Leslie, in the article Climate of the Encyclopædia Britannica, and suppose the density to be augmented, by the effect of cold, in the proportion of 1 to $1 + n \left(\frac{1}{z} - z\right)$, n being somewhat less than $\frac{1}{16}$; and since the density is as the pressure and the comparative specific

gravity conjointly, we have $z = y\left(1+n\left[\frac{1}{z} - z\right]\right)$, $\frac{z}{y} = 1 + \frac{n}{z} - nz$, $\mathrm{d}\frac{z}{y} = \frac{\mathrm{d}z}{y} - \frac{z\mathrm{d}y}{yy} = -\frac{n\mathrm{d}z}{zz} - n\mathrm{d}z$, and $\frac{\mathrm{d}y}{\mathrm{d}z} = \frac{y}{z} + \frac{nyy}{z^3} + \frac{nyy}{z}$; consequently $\frac{\mathrm{d}x}{\mathrm{d}r} = \frac{v}{mpsz}\left(\frac{y}{z} + \frac{nyy}{z^3} + \frac{nny}{z}\right)$, $\left(\frac{\mathrm{d}y}{y\mathrm{d}r}\right.$ $= -\frac{mz}{y}.\frac{\mathrm{d}x}{\mathrm{d}r} = -\frac{v}{psz} - \frac{nvy}{psz^3} - \frac{nvy}{psz}$. But instead of retracing the steps of the calculation with these corrections only, it will be more satisfactory to extend the general theorem somewhat further without confining it to a particular law of temperature.

5. For this purpose we may make $\frac{\mathrm{d}y}{\mathrm{d}z} = \zeta$, $\frac{\mathrm{d}\zeta}{\mathrm{d}r} = \zeta'$, $\frac{\mathrm{d}\zeta'}{\mathrm{d}r} = \zeta''$, and we shall have, for computing the coefficients of the series $vr + \frac{\mathrm{d}v}{\mathrm{d}r}.\frac{rr}{2} + \ldots$, the values

$$\mathrm{d}r = \frac{\mathrm{d}u}{v};$$

$$\frac{\mathrm{d}u}{\mathrm{d}r} = v;$$

$$\frac{\mathrm{d}x}{\mathrm{d}r} = \frac{\zeta v}{mpsz};$$

$$\frac{\mathrm{d}z}{\mathrm{d}r} = \frac{-\mathrm{d}u}{ps\mathrm{d}r} = \frac{-v}{ps};$$

$$\frac{\mathrm{d}y}{\mathrm{d}r} = \frac{\zeta\mathrm{d}z}{\mathrm{d}r} = \frac{-\zeta v}{ps};$$

$$\frac{\mathrm{d}v}{\mathrm{d}r} = \frac{\zeta x}{mpsz} - u;$$

$\mathrm{d}\frac{\mathrm{d}v}{\mathrm{d}r} = \mathrm{d}\zeta.\frac{x}{mpsz} + \mathrm{d}x.\frac{\zeta}{mpsz} - \mathrm{d}z\frac{\zeta x}{mpsz^2} - v\mathrm{d}r$; and $\frac{\mathrm{dd}v}{\mathrm{d}r^2} = \frac{\zeta' x}{mpsz} + \frac{\zeta^2 v}{m^2p^2s^2z^2} + \frac{\zeta xv}{mp^2s^2z^2} - v$. Now since m is about 766, it is obvious that the second term, containing its square, may be neglected in comparison with the third, since the other quantities concerned in these terms can never differ materially from each other: for the same reason the term v may be omitted, as not being divided by p, and u may be considered as equal to s, and its fluxion neglected, as well as that of x, which may be called $= 1$; and we may proceed to take the fluxion of $\frac{\mathrm{dd}v}{\mathrm{d}r} = \frac{\zeta' x}{mpsz} + \frac{\zeta xv}{mp^2s^2z^2} = \frac{\zeta'}{mpsz} + \frac{\zeta v}{mp^2s^2z^2}$; whence $\mathrm{d}\frac{\mathrm{dd}v}{\mathrm{d}r^2} = \frac{\mathrm{d}\zeta'}{mpsz} -$

$\frac{\zeta' dz}{mpsz^2} + \frac{\zeta dv + v d\zeta}{mp^2 s^2 z^2} - \frac{2\zeta v dz}{mp^2 s^2 z^3}$; and $\frac{d^3 v}{dr^3} = \frac{\zeta''}{mpsz} + \frac{\zeta' v}{mp^2 s^2 z^2} + \frac{\zeta}{m^2 p^2 s^2 z^2}.$ $\left(\frac{\zeta}{mpsz} - s\right) + \frac{\zeta' v}{mp^2 s^2 z^2} + \frac{2\zeta v^2}{mp^3 s^3 z^3}$. It will now be convenient to divide ζ'' into the two portions $\zeta''_{,}$ and $\zeta''_{,,} v^2$, in order to obtain that part of the sixth term which is independent of v; the fourth will then become $\frac{d^3 v}{dr^3} = \frac{\zeta''_{,}}{mpsz} + \frac{\zeta}{mp^2 s^2 z^2} \cdot \left(\frac{\zeta}{mpsz} - s\right) + \left(\frac{\zeta''_{,,}}{mpsz} + \frac{2\zeta'}{vmp^2 s^2 z^2} + \frac{2\zeta}{mp^3 s^3 z^3}\right) v^2$. The whole of the fluxion of the former part will contain v, which will disappear again in the next term, being changed into dv, and the v^2 of the second part will become $2 \frac{dv^2}{dr^2}$ in the sixth term. We shall therefore have for the case of the horizontal refraction, when $z = 1$ and $s = 1$, $\frac{d^3 v}{dr^3} = \left(\frac{d\zeta''_{,}}{mpdr} - \frac{\zeta''_{,}}{mp} \cdot \frac{dz}{dr} + \frac{\zeta'}{mp^2} \cdot \frac{dv}{dr} - \frac{2\zeta dz}{mp^2 dr} \cdot \frac{dv}{dr} + \frac{\zeta}{mp^2} \cdot \frac{ddv}{dr^2}\right)$ $\frac{dv}{vdr} + 2 \frac{dv^2}{dr^2}\left(\frac{\zeta''_{,,}}{mp} + \frac{2\zeta'}{vmp^2} + \frac{2\zeta}{mp^3}\right)$.

It is obvious, that since $\frac{ddv}{dr^2} = \left(\frac{\zeta'}{v} \cdot \frac{1}{mpsz} + \frac{\zeta}{mp^2 s^2 z}\right) v$, the quantity $\zeta''_{,}$ must be derived from it by taking the fluxion with respect to v only, and must be equal to $\frac{ddv}{dr^2} \cdot \frac{dv}{vdr}$, which is the product of the second and third coefficients. The fluxion of this quantity, $d\zeta''_{,}$ is also capable of a simpler expression; for since ζ' will in general be divisible by v, $\zeta'' = \frac{d'\zeta'}{dv} \cdot \frac{dv}{dr} = \frac{\zeta'}{v} \cdot \frac{dv}{dr}$, and $d\frac{\zeta'}{v} = \frac{d\zeta'}{v} - \frac{\zeta' dv}{vv}$; whence $\frac{d\zeta''_{,}}{dr} = \frac{d\zeta'}{vdr} \cdot \frac{dv}{dr} - \frac{\zeta'}{vv} \cdot \frac{dv^2}{dr^2} + \frac{\zeta'}{v} \cdot \frac{d^2 v}{dr^2} =$ $\frac{\zeta''}{v} \cdot \frac{dv}{dr} - \frac{\zeta''}{v} \frac{dv}{dr} + \frac{\zeta'}{v} \cdot \frac{d^2 v}{dr^2} = \frac{\zeta''_{,,} v^2}{v} \cdot \frac{dv}{dr} + \frac{\zeta'}{v} \cdot \frac{d^2 v}{dr^2} = \zeta''_{,,} v \cdot \frac{dv}{dr} + \frac{\zeta'}{v} \frac{d^2 v}{dr^2}$. Consequently

$$\frac{d^3 v}{r^3} = \left(\frac{\zeta''_{,,}}{mp} \cdot \frac{dv}{dr} + \frac{\zeta'}{v^2 mp} \cdot \frac{d^2 v}{dr^2} + \frac{\zeta'}{vmp^2} \cdot \frac{dv}{dr} + \frac{\zeta'}{vmp^2} \cdot \frac{dv}{dr} + \frac{2\zeta}{mp^3} \cdot \frac{dv}{dr} + \frac{\zeta}{vmp^2} \cdot \frac{ddv}{dr^2}\right) \frac{dv}{dr} + \frac{dv^2}{dr^2} \left(\frac{2\zeta''_{,,}}{mp} + \frac{4\zeta'}{vmp^2} + \frac{4\zeta}{mp^3}\right) = \frac{dv^2}{dr^2} \left(\frac{3\zeta''}{mp} + \frac{6\zeta'}{vmp^2} + \frac{6\zeta}{mp^3}\right) + \frac{dv}{dr}\left(\frac{\zeta'}{v} + \frac{\zeta}{p}\right) \frac{ddv}{vmpdr^2}.$$

6. We may next proceed to substitute, in these general expressions, the values derived from the various laws which may

be supposed to govern the variations of temperature: observing first that in general

$m = 766$, $p = .000\,2825 = \frac{1}{3540}$; whence

$\frac{1}{mp} = 4.621$, $\frac{1}{m^2p^2} = 21.3536$, $\frac{1}{mp^2} = 16358$, $\frac{1}{m^2p^3} = 72052$,

$\frac{1}{mp^3} = 57\,907\,320$.

7. (A) If the temperature were uniform, we should have $y = z$, $\mathrm{d}y = \mathrm{d}z$, $\zeta = 1$, $\zeta' = 0$, $\zeta'' = 0$, and $\frac{\mathrm{d}v}{\mathrm{d}r} = \frac{1}{mps} - s = \frac{4.621}{s} - s$; and when $s = 1$, 3.621.

$$\frac{\mathrm{d}^2v}{\mathrm{d}r^2} = \frac{v}{mp^2s^2}$$

$$\frac{\mathrm{d}^3v}{\mathrm{d}r^3} = \frac{1}{mp^2s^2}\left(\frac{1}{mps} - s\right) + \frac{2v^2}{mp^3s^3}; \text{ or if } v = 0,\ 16358 \times 3.621.$$

$$\frac{\mathrm{d}^5v}{\mathrm{d}r^5} = \left(\frac{1}{mp} - 1\right)^2 \frac{6}{mp^3} + \left(\frac{1}{mp} - 1\right) . \frac{1}{m^2p^4} = 3.621\ (6 \times 57907320 \times 3.621 + 16358^2) = 5524050000;$$

$\frac{1}{720}$ of which is 7672300. Hence, for $s = 1$, we have the equation $.0002825 = 1.8105\ r^2 + 2467\ r^4 + 7672300\ r^6 + \ldots$, in which, if we put $r^2 = .000130$, we shall have $.0002825 = .0002939 + \ldots$; which is too much: then taking $r^2 = .000120$, we have $.0002825 = .00021726 + .00003552 + .00001315 + [.00001647]$: and this is somewhat too great a remainder; for the quotients of the terms being 6, 3..., the remainder ought not to exceed the last term; so that r^2 must be about .000121, and $r = .0110$, or 37′ 50″, which is too great by about one ninth. By the assistance of this series we might easily compute the refraction upon the hypothesis of Professor Bessel, who supposes the variation of density to follow the same law as if the temperature were uniform, but alters the value of m so as to accommodate it to the actual magnitude of the refraction in low altitudes.

(B) In Professor Leslie's hypothesis, we have

$$n = \frac{45}{500} = .09$$

$\zeta = \frac{y}{z} + \frac{nyy}{z} + \frac{nyy}{z^3}$; the initial value $\zeta = 1 + 2n = 1.18$

$$' = \frac{v}{ps}\left(\frac{y}{x} + \frac{nyy}{z^3} + \frac{3nyy}{z^4}\right) - \frac{v\zeta}{ps}\left(\frac{1}{z} + \frac{2ny}{z} + \frac{2ny}{z^3}\right)$$

$$\zeta'' = \frac{v}{ps}(1+4n)\,(1-\zeta'') = \frac{-v}{ps}(2n+8n^2)$$

$$\frac{d^2v}{dr^2} = \frac{1+2n-(2n+8n^2)}{mp^2s^2}\,v = \frac{1-8n^2}{mp^2s^2}v.$$

$$\zeta'' = \frac{d'\zeta'}{dv}\cdot\frac{dv}{dr} + \frac{d'\zeta'}{dy}\cdot\frac{dy}{dr} + \frac{d'\zeta'}{dz}\cdot\frac{dz}{dr} + \frac{d'\zeta'}{d\zeta}\cdot\frac{d\zeta}{dr} = \zeta''_{,} + \zeta''_{,}\,v^2$$

$$\zeta''_{,} = \frac{d'\zeta'}{dv}\cdot\frac{dv}{dr} = \frac{\zeta'}{v}\cdot\frac{dv}{dr} = \frac{\zeta'}{v}\left(\frac{1+2n}{mpsz} - s\right)$$

$$\zeta''_{,,}\,v^2 = \frac{d'\zeta'}{dv}\cdot\frac{-\zeta v}{ps} + \frac{d'\zeta'}{dz}\cdot\frac{-v}{ps} + \frac{d'\zeta'}{d\zeta}\cdot\zeta'$$

$$\zeta''_{,,}\,v^2 = \left\{\frac{v}{ps}(1+8n) - \frac{\zeta v}{ps}(4n)\right\}\cdot\frac{-\zeta v}{ps} - \frac{v}{ps}\left\{\frac{-v}{ps}(2+14n) + \frac{v\zeta}{ps}(1+8n)\right\} + \frac{v^2}{p^2s^2}(1+4n)\,(2n+8n^2) = \frac{v^2}{p^2s^2}\cdot\left(-\zeta(1+8n) + \zeta^2(4n) + 2 + 14n - \zeta(1+8n) + 2n + 16n^2 + 32n^3\right)$$

$$= \frac{v^2}{p^2s^2}(-1-8n-2n-16n^2+4n+16n^2+16n^3+2+14n-1-8n-2n-16n^2+2n+16n^2+32n^3)$$

$$= \frac{v^2}{p^2s^2}\cdot(48n^3)$$

$$\frac{d^3v}{dr^3} = \frac{1}{mp^2s^2}\left\{-(2n+8n^2)\left(\frac{1+2n}{mps} - s\right) + \zeta\left(\frac{1+2n}{mps} - s\right)\right\} + \frac{v^2}{mp^3s^3} - (48n^3-(4n+16n^2)+2+4n)$$

$$= \frac{1-8n^2}{mp^2s^2}\left(\frac{1+2n}{mps} - s\right) + \frac{2+16n^2+48n^3}{mp^3s^3}v^2.$$

$$\frac{d^5v}{dr^5} = \left(\frac{1+2n}{mp} - 1\right)^2\cdot\frac{1}{mp^3}(144n^3-12n-48n^2+6+12n) + \left(\frac{1+2n}{mp} - 1\right)\cdot\frac{1-8n^2}{m^2p^3}\left(\frac{1+2n}{p} - \frac{2n+8n^2}{p}\right) = \left(\frac{1+2n}{mp} - 1\right)^2\frac{1}{mp^3}(6-48n^2+144n^3) + \left(\frac{1+2n}{mp} - 1\right)\left(\frac{1-8n^2}{mp^2}\right)^2.$$

We have then, for the case of horizontal refraction, $\frac{dv}{dr} = 4.453 = 2 \times 2.2265$; $\frac{d^3v}{dr^3} = \frac{.9352}{mp^2} \times 4.453 = 68112 = 24 \times 2838$, and $\frac{d^5v}{dr^5} = (4.453)^2 \times 57907320 \times 5.7162 + 4.453 \times 15296^2 = 7657200\ 000 = 720 \times 10635000$: consequently, $.0002825 = 2.2265\,r^2 + 2838\,r^4 + 10635000\,r^6$; now if $r^2 = .0001$, we have $.0002825 = .00022265 + .00002838 +$

.000010635 [+.000020935] : consequently, .0001 is too little for r^2, and we may try .00011, giving .0002825 = .00024491 +.00003434 + .00001420 [−.00001095]. But in order to keep up the probable sequence of the progression, the remainder should be about equal to the last term, or about .000011, and .0000209 should have been diminished by about .00001 instead of .0000318; so that we must take .000103 as the true value of r^2 on this hypothesis, and $r = 34' \, 53''$, which is too great by about 1′; a difference by far too considerable to be attributed to the errors of observation only; and we must infer, that the law of temperature, obtained from the height of the line of congelation, is not correctly true, if applied to elevations remote from the earth's surface. If indeed this law were fully established, and capable of being applied, with any little modification, to the exact computation of the refraction, it would be necessary, for the lowest altitudes, either to compute a greater number of the fluxional coefficients, or to divide the refraction into two or more parts, and determine the successive changes of density required for each of them. We should also have for finding, on this hypothesis, the height x, corresponding to the pressure y and the density z, the expression $mx - m = 1 - \frac{y}{z} + \frac{n}{q} \,\mathrm{hl}\, \frac{2zz + qy(1-z)}{2zz - qy(1-z)}$; y being $= \frac{zz}{z + n - nzz}$, and $q^2 = 1 + 4n^2$: and the actual state of the atmosphere would probably be very well represented by this formula, taking $n = .1$ or $.11$, rather than .09.

(C) Professor Bessel's hypothesis is also found to make the horizontal refraction too great. Mr. Laplace's formula, which affords a very correct determination of the refraction, is said to agree sufficiently well with direct observation also; but, in fact, this formula gives a depression considerably greater than was observed by Gay Lussac, in the only case which is adduced in its support; and the progressive depression follows a law which appears to be opposite to that of nature, the temperature varying less rapidly at greater than at smaller heights, while the observations of Humboldt and others seem to prove that in nature they vary more rapidly. Notwithstanding, therefore, the ingenuity, and even utility of Mr. Laplace's formula, it can

only be considered as an optical hypothesis, and we are equally at liberty to employ any other hypothesis which represents the results with equal accuracy; or even to correct our formulas by comparison with astronomical observations only, without assigning the precise law of temperature implied by them.

(D) We may compute the effect of a temperature supposed to vary uniformly with the height, by making $z = y\,(1+tx-t)$, or $= yx^t$; we have then $\frac{z}{y} = 1+tx-t$, or x^t, and $\mathrm{d}\,\frac{z}{y} = \frac{\mathrm{d}z}{y} - \frac{z\mathrm{d}y}{yy}$ $= t\mathrm{d}x$, or $= tx^{t-1}\mathrm{d}x$, which are initially the same. But $t\mathrm{d}x = \frac{-t\mathrm{d}y}{mz}$, and $\frac{\mathrm{d}z}{y} = \frac{z\mathrm{d}y}{yy} - \frac{t\mathrm{d}y}{mz}$, whence $\frac{\mathrm{d}z}{\mathrm{d}y} = \frac{z}{y} - \frac{ty}{mz} = \frac{mzz-tyy}{myz}$, and $\frac{\mathrm{d}\,y}{\mathrm{d}\,z}$ $= \zeta = \frac{myz}{mzz-tyy}$; consequently $\mathrm{d}\zeta = \frac{my\mathrm{d}z+mz\mathrm{d}y}{mzz-tyy} - 2myz.\frac{mz\mathrm{d}z-ty\mathrm{d}y}{(mzz-tyy)^2}$; and initially $\zeta = \frac{m}{m-t}$, and $\zeta' = \mathrm{d}\,\frac{\mathrm{d}\zeta}{\mathrm{d}r} = \left(\frac{m+m\zeta}{m-t} - 2m\,\frac{m-t\zeta}{(m-t)^2}\right)\frac{-v}{ps} =$ $\left(\zeta+\zeta^2-2\,\zeta^2+\frac{2t\zeta^2}{m-t}\right)\frac{-v}{ps} = \left(\zeta-\zeta^2+2\zeta^2.\,\frac{t}{m-t}\right)\frac{-v}{p\,s} = \Big\{\zeta -$ $\zeta^2+2\zeta^2\,(\zeta-1)\Big\}\frac{-v}{ps} = \Big\{(2\zeta^2-\zeta)(\zeta-1)\Big\}\frac{-v}{ps} = \zeta\,(\zeta-1).(2\zeta-1)$ $\frac{-v}{ps}$. Now, if we suppose the temperature to vary 1° in 300 feet, we have $\frac{1}{500}\cdot\frac{1}{300} = \frac{1}{150000}$, for the variation of density depending on temperature in $\frac{1}{20900000}$ of the earth's radius x; hence t should be 139, and $\zeta = \frac{766}{766-139} = 1\,.26$, whence $\frac{\zeta}{m\,p} = 5\,.822$, while the phenomena of refraction require this quantity to be about 6. Thus, in Bradley's approximation, we first take $r = \frac{p\,s}{v}$, and then $r = p\tan\left(\mathrm{ZD}-\frac{3ps}{v}\right) = p\,\left(\frac{s}{v} - \frac{3ps}{v}\right.$ $\left.\left(1+\frac{ss}{vv}\right)\right)$ very nearly, or $r = \frac{ps}{v} - \frac{3p^2s}{v} - \frac{3p^2s^3}{v^3}$, and $vr = ps - \frac{3r^2}{s} -$ $3r^2s$, or, while s remains small, $ps = vr + 3\frac{r^2}{s}$, which is sufficiently accurate near the zenith. If we make $\frac{\zeta}{mp} = 6$, we shall have $\zeta = 1\,.3$, and $t = 176$, which is equivalent to a depression of a degree of Fahrenheit in 227 feet: we shall then have, for ζ', $-1.3\times .3\times 1.6\,\frac{v}{ps} = -\,.624\,\frac{v}{ps}$, and $\frac{\mathrm{d}^2v}{\mathrm{d}r^2} = (1.3-.624)\,\frac{v}{p^2s^2}$

$= .676 \frac{v}{mp^2s^2} = .676 \times 16358$, and $\frac{1}{6}$ of this, or 1854, is the coefficient of the third term. With the same value of ζ, taking $n = .15$, this coefficient would become, upon a hypothesis similar to Professor Leslie's, 2236.

8. It is not possible, in the present state of our knowledge of the subject, to determine, from observation, either the refraction with sufficient accuracy to enable us to compute from it the law of the variation of temperature, or the variation of temperature with sufficient accuracy for computing the refraction. Considering, indeed, how improbable it is that the upper regions of the atmosphere should be of the same temperature as the surface of the hills on the same general level, we could scarcely expect the agreement to be more complete than these computations make it; and it is perfectly possible either that t may be as great, at 176, or that n may be .15: but we cannot determine from the observed refraction which of the laws of variation is capable of representing it with the greatest accuracy: much less should we be justified in believing, because Mr. Laplace's formula happens to represent the refraction very accurately, that the temperature varies the less rapidly as we ascend higher. It is, however, perfectly justifiable, for the purposes of astronomy, to adopt the form of the equation which is shown by these examples to be converging, and to correct the coefficients by an immediate comparison with observation; and in this manner it has been found that the formula employed in the *Nautical Almanac* is abundantly sufficient for the purposes to which it is applied. This formula is $.0002825 = v \frac{r}{s} + (2.47 + .5v^2) \frac{r^2}{s^2} + 3600\, v \frac{r^3}{s^3} + 3600\,(1.235 + .25\, v^2) \frac{r^4}{s^4}$; its results are almost identical with those of the French tables, except in the immediate neighbourhood of the horizon. But the effect of a difference of temperature at the place of observation is not so correctly represented by any of the tables commonly employed, and requires to be separately examined.

9. The terrestrial refraction may be most easily determined by an immediate comparison with the angle subtended at the

earth's centre, the fluxion of which is $\frac{u\mathrm{d}x}{vx}$, and $\frac{u\mathrm{d}x}{vx\mathrm{d}r}$ is initially the first part of the coefficient of the second term of the series already obtained, and is equal to about 6; so that this angle, while it remains small, is six times the refraction: commonly, however, the refraction in the neighbourhood of the earth's surface is somewhat less than in this proportion.

10. The effects of barometrical and thermometrical changes may be deduced from the fluxion of the equation, if we make m, p, and n, or rather t, vary: and for this purpose it will be convenient to employ the form $ps = vr + \left(\frac{1}{2(m-t)ps} - \frac{s}{2}\right) r^2$, the value of the fraction, if we neglect the subsequent terms, becoming 3.41; and this expression is sufficiently accurate for calculating the whole refraction, except for altitudes of a few degrees. Now the fluxion of $p = v\frac{r}{s} + \left(\frac{1}{2(m-t)p} - \frac{ss}{2}\right)\frac{rr}{ss}$, which we may call $p = v\frac{r}{s} + \left(\frac{1}{w} - \frac{ss}{2}\right)\frac{rr}{ss}$, is $\mathrm{d}p = \left(\frac{v}{s} + \left(\frac{1}{w} - \frac{ss}{2}\right)\frac{2r}{ss}\right) dr - \frac{rr}{ssw}\left(\frac{dm - dt}{m - t} + \frac{\mathrm{d}p}{p}\right)$, the coefficient of dr being equal to $\frac{2p}{r} - \frac{v}{s}$; and $\left(2p - \frac{rv}{s}\right)\frac{\mathrm{d}r}{r} = \left(p + \frac{rr}{ssw}\right)\frac{\mathrm{d}p}{p} + \frac{rr}{ssw}\left(\frac{\mathrm{d}m - \mathrm{d}t}{m-t}\right)$; $\frac{1}{w}$ being 3.41, and $m-t$, on this supposition, 519. The proportional variation of p, or $\frac{\mathrm{d}p}{p}$, will be $\frac{1}{500}$ for every degree that the thermometer varies from 50°; and $\frac{dm}{m}$ being also $\frac{1}{500}$, $\frac{dm}{m-t}$ will be $\frac{766}{519 \times 500} = .003$. The variation of t can only be determined from conjecture; but supposing the alteration of temperature to cease at the height of about 4 miles, it must increase, with every degree that the thermometer rises at the earth's surface, about $\frac{1}{120}$, and $\frac{dt}{t}$ being $\frac{1}{120}$, $\frac{dt}{m-t}$ will be $\frac{247}{519 \times 120} = .004$. The alterations of the barometer will affect p only, $\frac{dp}{p}$ being $\frac{1}{30}$ for every inch above or below 30. It is evident, since $m = \frac{3958 \times 5280 \times 12}{13.57\, hd}$, h being the height of the barometer, and d the bulk of air compared to that of water, that m must diminish, as well as p, when the temperature increases; and the correction for t being

subtractive, the three variations will co-operate in their effects; but the proportion will be somewhat different from that of the simple densities. If we preferred the expression derived from Professor Leslie's hypothesis, we should merely have to substitute $\frac{2dn}{1+2n}$ for $\frac{dt}{m-t}$, and the variation depending on the *law* of temperature would become about $\frac{2}{3}$ as great. It must, however, be limited to such changes as affect the lower regions of the atmosphere only, its "argument" being the deviation from the mean temperature of the latitude; but even in this form it cannot be satisfactorily applied to the observations at present existing; although it appears to be amply sufficient to explain the irregularities of terrestrial refraction, as well as the uncommon increase of horizontal refraction in very cold countries: and we may even derive from all these considerations a correction of at least half a second, or perhaps of a whole second, for the sun's altitude at the winter solstice, tending to remove the discordance, which has so often been found, in the results of some of the most accurate observations of the obliquity of the ecliptic.

The preceding Memoir was severely criticised in the September number of the Philosophical Magazine for 1821 (vol. xxxviii., p. 167), by Mr. Ivory, who disputed the assumptions which were involved in the investigation of the series, denied the sufficiency of its convergency, at least for low altitudes, and asserted that the formula and the table of refraction deduced from it were entirely empirical: he contended also that the method of series was only resorted to in the infancy of analytical science, when other expedients were unknown, and that it was necessarily inferior in elegance and power to the methods which Kramp, Laplace, Bessel, and other analysts had employed in these inquiries.

To these observations Dr. Young replied in the Quarterly Journal of Science for 1822 (vol. xii., p. 390), in a tone of considerable severity. By a modification of his method, dividing the operation into two parts, he showed that the series might be made sufficiently convergent, even for extreme cases; and proceeded farther to prove its capacity, by showing in what manner the actual density of the air, at a given height, might be deduced from a table of refractions. As the substance of this *Apology*, as it was termed, is involved in the articles which follow, it has not been thought necessary to reprint it.—*Note by the Editor.*

No. XXX.

AN EXTENSION OF THE INVERSE SERIES FOR

THE COMPUTATION OF REFRACTION,

TOGETHER WITH A DIRECT SOLUTION OF THE PROBLEM.

From 'Brande's Quarterly Journal' for 1823, vol. xvi. p. 139.

CONSIDERING the acknowledged and increasing importance of the accurate determination of astronomical refractions, it may not be thought superfluous to attempt to confirm and extend the mode of computation, which has been adopted for the Table of Refractions printed in the 'Nautical Almanac,' and at the same time to compare its results, in the most unfavourable case for its application, with those of the direct method, which, in that case only, are very readily obtained.

If r be the refraction, z the density, $= 1 - \chi$
y the pressure, x the distance from the centre,
u the perpendicular falling from the centre on the direction of the ray,
v the distance of this perpendicular from the point of refraction,
s the initial value of u, or $\check{u}$; we shall have (No. XXIX. p. 30.)

$\mathrm{d}r = \frac{\mathrm{d}u}{v}$ $\mathrm{d}y = -mz\mathrm{d}x$,

$u = \frac{1+p}{1+pz}s = (1+p-pz)s = (1+p\chi)s$, and, p being a very small fraction,

$v^2 = x^2 - u^2 = x^2 - s^2 - 2p\chi s^2$, $\frac{\mathrm{d}y}{\mathrm{d}z} = \zeta$

$\frac{\mathrm{d}z}{\mathrm{d}r} = \frac{-v}{ps}$ $\frac{\mathrm{d}y}{\mathrm{d}r} = \frac{-\zeta v}{ps}$

$\frac{\mathrm{d}x}{\mathrm{d}r} = \frac{\zeta v}{mpsz}$. We may then put, in order the better to observe the progress of the subsequent operations,

$$\frac{dv}{dr} = Z$$

$$\frac{d^2v}{dr^2} = * + Yv$$

$$\frac{d^3v}{dr^3} = Z' + * + Xv^2$$

$$\frac{d^4v}{dr^4} = * + Y'v + * + Vv^3$$

$$\frac{d^5v}{dr^5} = Z'' + * + X'v^2 + * + Uv^4$$

$$\frac{d^6v}{dr^6} = * + Y''v + * + V'v^3 + \dots$$

$$\frac{d^7v}{dr^7} = Z''' + * + X''v^2 + \dots$$

$$\frac{d^8v}{dr^8} = * + Y'''v + \dots$$

$$\frac{d^9v}{dr^9} = Z'''' + \dots$$

It will be convenient to denote the successive results of the differentiation of any quantity, Z, Y, X, with respect to y or z, which introduces a new power of v, by Z_1v, Z_2v^2, Y_1v, Y_2v^2, and so forth; we shall then have

$$Z = Z \qquad Z' = YZ$$

$$Y = Z_1 \qquad Y' = 2XZ + Z'_1 = (2Y_1Z) + (Y_1Z + Y^2) = 3Y_1Z + Y^2$$

$$X = Y_1 \qquad X' = 3VZ + Y'_1 = (3Y_2Z) + (3Y_2Z + 3Y_1Y + 2Y_1Y)$$

$$V = X_1 = Y_2 \qquad = 6Y_2Z + 5Y_1Y$$

$$U = Y_3 \qquad V' = 4UZ + X'_1 = (4Y_3Z) + (6Y_3Z + 6Y_2Y) + (5Y_2Y + 5Y_1^2) = 10Y_3Z + 11Y_2Y + 5Y_1^2$$

$$Z'' = Y'Z = 3Y_1Z^2 + Y^2Z$$

$$Y'' = 2X'Z + Z''_1 = 12Y_2Z^2 + 10Y_1YZ + (3Y_2Z^2 + 6Y_1YZ + 2Y_1YZ + Y^3) = 15Y_2Z^2 + 18Y_2YZ + Y^3$$

$$X'' = 3V'Z + Y''_1 = (30Y_3Z^2 + 33Y_2YZ + 15Y_1^2Z) + (15Y_3Z^2 + 30Y_2YZ) + (18Y_1^2Z + 18Y_2YZ + 18Y_1Y^2) + (3Y_1Y^2)$$

$$= 45Y_3Z^2 + 81Y_2YZ + 33Y_1^2Z + 21Y_1Y^2$$

$$Z''' = Y''Z = 15Y_2Z^3 + 18Y_1YZ^2 + Y^3Z$$

$$Y'''=2X''Z+Z'''_1=(90\,Y_3Z^3+162\,Y_2YZ^2+66\,Y_1^2Z^2+42\,Y_1Y^2Z)$$
$$+(15\,Y_3Z^3+45\,Y_2YZ^2)+(18\,Y_1^2Z^2+36\,Y_1Y^2Z$$
$$+18\,Y_2YZ^2) \qquad +(3\,Y_1Y^2Z$$
$$+Y^4$$
$$=105\,Y_3Z^3+225\,Y_2YZ^2+84\,Y_1^2Z^2+81\,Y_1Y^2Z$$
$$+Y^4$$

$Z'''' = Y'''Z.$

If we now select, from these general values of the coefficients, those which are concerned in the horizontal refraction when $v = 0$, and $s = 1$, we shall have, instead of $ps = \frac{\mathrm{d}v}{\mathrm{d}r}\frac{r^2}{2} + \ldots$, putting $r' = \frac{r^2}{p}$,

$$1 = \frac{\mathrm{d}v}{\mathrm{d}r}\frac{r'}{2} + \frac{\mathrm{d}^3v}{\mathrm{d}r^3}\,p\,\frac{r'^2}{24} + \frac{\mathrm{d}^5v}{\mathrm{d}r^5}\,p^2\,\frac{r'^3}{720} + \ldots, \text{ or}$$

$$1 = \frac{Z}{2}r' + \frac{Z'}{24}pr'^2 + \frac{Z''}{720}p^2r'^3 + \frac{Z'''}{40320}p^3r'^4 + \frac{Z''''}{3628800}p^4r'^5 + \ldots,$$

in which we must substitute the values of $Z^{\cdots}$, derived from the particular hypothesis respecting the constitution of the atmosphere that we may choose to adopt.

Example A. The simplest application, that can be made of this series, is to put, instead of Professor Leslie's hypothesis of $z^2 = y\,(n + z - nz^2)$, merely $z^2 = y$, whence $\zeta = \frac{\mathrm{d}y}{\mathrm{d}z} = 2z$, and $Z = \frac{2z}{mpsz} - s = \frac{2}{mps} - s$; consequently $\mathrm{d}Z = 0$, and the series stops at the second term, assuming precisely the form which has been actually employed, as an approximation for determining the effect of a change of temperature. (No. XXIX.) To inquire what would be the physical conditions, that would be implied by this equation, would be to anticipate the contents of a very elaborate memoir, which is probably now in the press, and in which the author has deduced some very convenient and elegant expressions, when considered merely in a mathematical point of view, from a law of condensation which will scarcely be admitted by natural philosophers in general, as applicable to the phenomena in their whole extent.*

* This refers to a very elaborate Memoir on Refraction by Mr. Ivory, which had been read to the Royal Society, but which was not yet published: it appeared in the Philosophical Transactions for 1823.—*Note by the Editor.*

Example B. We may also obtain a finite inverse series, nearly resembling that of the 'Nautical Almanac,' from the equation $y = .57 + .43z^3$, which is obviously impossible in nature, since it supposes a constant pressure after the density has vanished. A result, however, nearly identical, may be deduced from the supposition $y = 3.4z^2 - 4.1z^3 + 1.7z^4$, which implies an atmosphere terminating at the height of about 14 miles; although the series thus obtained would extend to a fifth term, instead of ending at the fourth, but without producing any material difference in the result. Considering, indeed, the analogy between logarithms and high powers, it is not improbable that the true value of y might be very correctly expressed by a series of this form, however complicated it might appear at first sight. The value of x and the height $h = 20900000\,(x-1)$, in feet, might be found from the fluxion $\mathrm{d}x = \frac{-\mathrm{d}y}{mz} = 6.8\mathrm{d}z - 12.3z\mathrm{d}z + 6.8\,z^2\mathrm{d}z$, and $x - 1 = \frac{1}{m}(-6.8z + 6.15z^2 - 2.27z^3 + 2.92)$, or $h = 27300\,(2.92 - 6.8z + 6.15z^2 - 2.27z^3)$; which becomes 21300, when the density z is reduced to $\frac{1}{2}$; and the pressure $y = .444$.

Example C. A. As the most unfavourable specimen of the application of this method, we may take the case of an equable temperature, at the horizon: and first suppose, with Laplace, that $m = 798$, and $\frac{1}{p} = 3403$, so that $\frac{1}{mp} = 4.2624$, $Z = \frac{\zeta}{mp} - 1 = 3.2624$, since z is here $= y$, and $\zeta = 1$; $Y = \frac{1}{mp} \cdot \frac{1}{p} = \frac{4.2624}{p}$, $Y_1 = \frac{2}{p}Y$, $Y_2 = \frac{3}{p}Y_1$, and $Y_3 = \frac{4}{p}Y_2$. Hence we have $YZ = \frac{13.9056}{p} = Z'$, and the equation becomes
$1 = 1.6312r' + .5794r'^2 + .4603r'^3 + .5093r'^4 + .6517\,r'^5 + \ldots$
Now the value of r' cannot be very accurately obtained from these coefficients, without a liberal employment of the method of logarithmic differences, finding the results derived by it from the first three, the middle three, and the last three terms, and comparing these with each other; and in this manner it seems natural to suppose that we might easily come within

about $\frac{1}{500}$ of the truth.* The best inference of this kind, however, that has been obtained, was $r = 40' \, 15''$, which is too much by about $\frac{1}{130}$.

If still greater accuracy were required, we might compute a greater number of the coefficients of the series, or we might separate the computation into two or more parts; but it would be a little troublesome to adapt the new values of Z, and its derivatives, either to the diminished magnitude of the density z, or to a value of p diminished in the same proportion; so that if the actual density at the time in question were called unity, the refractive density might still be truly represented by $1 + p$; observing also to make the remaining portion Δz equal to unity: and in this case the values of Z, and of its powers only, would require to be changed in the subsequent computation. This operation has been somewhat more negligently performed in the Memoir just referred to; but its object then was merely to show the convergence of the series, and the object was obtained.

B. With the values $m = 766$, $p = \frac{1}{3540}$, and $\frac{1}{mp} = 4.621$, we obtain $Z = 3.621$, $Y = \frac{4.621}{p}$, $Z' = YZ = 17.190$, $Z'' = \frac{6}{p} YZ^2 + Y^2 Z = \frac{452.916}{p^2}$, $Z''' = \frac{31290}{p^3}$, and $Z'''' = \frac{3714095}{p^4}$; and for the equation $1 = \frac{Z}{2} r' + \frac{Z''}{24} pr'^2 + \ldots$, we have $1 = 1.8105 r' + .7162 r'^2 + .6290 r'^3 + .7760 r'^4 + 1.0231 r'^5 + \ldots$; and if we make $r' = 44$, we shall have the true sum 1.0460; if $r' = .43$, 1.0218, whence $r' = 4210$, and r appears to be $37' \, 29''$.

But if we wish to supply any real or imaginary deficiency of the inverse series, we may easily revert to a modification of the original solution of Taylor, who first applied to the problem of atmospherical refraction his very useful theorem for "integration by parts," as the process is sometimes now called, that is, $\int Z \mathrm{d} Y = \frac{\mathrm{d}Y}{\mathrm{d}X} \int Z \mathrm{d}X - \mathrm{d}\,\frac{\mathrm{d}Y}{\mathrm{d}X^2} \int^2 Z \mathrm{d}X^2 + \ldots$. Taking the fundamental equation for the refraction, $\mathrm{d}r = \frac{\mathrm{d}z}{v}$,

* See Brande's Quarterly Journal, vol. xii. p. 395.

and making first $Z = \frac{1}{v}$, $\mathrm{d}X = v\mathrm{d}v$, and $\mathrm{d}Y = \mathrm{d}z$, we have for $\int Z\mathrm{d}x$, $\int^2 Z\mathrm{d}x^2, \ldots$, v, $\frac{1}{3}v^3$, $\frac{1}{3.5}v^5, \ldots$, and for $\frac{\mathrm{d}Y}{\mathrm{d}X}, \ldots, \frac{\mathrm{d}z}{v\mathrm{d}v}$, $\mathrm{d}\frac{\mathrm{d}z}{v\mathrm{d}v} : v\mathrm{d}v, \ldots$; or secondly, making $\frac{\mathrm{d}z}{v} = \mathrm{d}\frac{z}{v} + \frac{z\mathrm{d}v}{vv}$, we have $\int\frac{\mathrm{d}z}{v} = \frac{z}{v} + \int\frac{z\mathrm{d}v}{vv}$, in which making $\mathrm{d}X$ again $= v\mathrm{d}v$, and $Z = z$, $\mathrm{d}Y$ being $= \frac{\mathrm{d}v}{vv}$, we have $\int\frac{\mathrm{d}z}{v} = \frac{z}{v} + \frac{1}{v^3}\int zv\mathrm{d}v + \frac{3}{v^5}\int^2 zv\mathrm{d}v\,.\,v\mathrm{d}v + \frac{3.5}{v^7}\ldots$. Now in all cases $v^2 = x^2 - u^2$, and $v\mathrm{d}v = x\mathrm{d}x - u\mathrm{d}u = x\mathrm{d}x + psu\mathrm{d}z$, and since $\mathrm{d}x = \frac{-\mathrm{d}y}{mz} = \frac{-\zeta\mathrm{d}z}{mz}$, we have $\frac{v\mathrm{d}v}{\mathrm{d}z} = \frac{-x\zeta}{mz} + psu = \frac{mpsuz - x\zeta}{mz}$, and $\frac{\mathrm{d}z}{v\mathrm{d}v} = \frac{mz}{mpsuz - x\zeta}$, whence $r = \int\frac{\mathrm{d}z}{v} = \frac{vmz}{mpsuz - x\zeta} - \frac{v^3}{3}\mathrm{d}\frac{vmz}{mpsuz - x\zeta}\,.\,\frac{1}{\mathrm{d}z}\,.\,\frac{mz}{mpsuz - x\zeta} + \ldots$; and from one or the other of the series thus obtained, we may always compute the value of r, taking the fluents from $z = 1$ to $z = 0$. But at the horizon, it will be easier to employ the particular fluent ${}^1\!\int_0 \frac{\mathrm{d}z}{\sqrt{\mathrm{hl}\,\frac{1}{z}}} = \sqrt{\pi}$, discovered by Euler, and still more elegantly demonstrated by Laplace, in the form ${}^{-\infty}\!\int_{\infty} e^{-xx}\,\mathrm{d}x = \sqrt{\pi}$: and the application of this proposition leads us to the integration of several fluents, which may be thus enumerated:

A. $${}^1\!\int_0 \frac{\mathrm{d}z}{\sqrt{\mathrm{hl}\,\frac{1}{z}}} = \sqrt{\pi} = \sqrt{3.141592}.$$

B. $${}^1\!\int_0 \frac{z^n\mathrm{d}z}{\sqrt{\mathrm{hl}\,\frac{1}{z}}} = {}^1\!\int_0 \frac{\sqrt{(n+1)}}{n+1}\,.\,\frac{\mathrm{d}y}{\sqrt{\mathrm{hl}\,\frac{1}{y}}} = \sqrt{\frac{\pi}{n+1}},$$ by putting $y = z^{n+1}$.

C. $$\int\frac{z^n\mathrm{d}z}{\mathrm{hl}^m\frac{1}{z}} = \frac{1}{m-1}\,.\,\frac{z^{n+1}}{\mathrm{hl}^m\frac{1}{z}} - \frac{n+1}{m-1}\int\frac{z^n\mathrm{d}z}{\mathrm{hl}^{m-1}\frac{1}{z}}.$$

i. $${}^1\!\int_0 \frac{\mathrm{d}z}{\mathrm{hl}^{\frac{3}{2}}\frac{1}{z}} = 2\,\frac{z}{\mathrm{hl}^{\frac{1}{2}}\frac{1}{z}} - 2\sqrt{\pi}\,;\quad {}^1\!\int_0 \frac{\mathrm{d}z}{\mathrm{hl}^{\frac{5}{2}}\frac{1}{z}} = \frac{2}{3}\,\frac{z}{\mathrm{hl}^{\frac{3}{2}}\frac{1}{z}} - \frac{4}{3}\,\frac{z}{\mathrm{hl}^{\frac{1}{2}}\frac{1}{z}} + \frac{4}{3}\sqrt{\pi}.$$

ii. $\int_0^1 \frac{z\mathrm{d}z}{\sqrt{\mathrm{hl}\,\frac{1}{z}}} = \sqrt{\frac{\pi}{2}}$; $\int_0^1 \frac{z\mathrm{d}z}{\mathrm{hl}^{\frac{3}{2}}\frac{1}{z}} = 2\,\frac{zz}{\mathrm{hl}^{\frac{1}{2}}\frac{1}{z}} - 4\sqrt{\frac{\pi}{2}}$;

$$\int_0^1 \frac{z\mathrm{d}z}{\mathrm{hl}^{\frac{5}{2}}\frac{1}{z}} = \frac{2}{3}\,\frac{zz}{\mathrm{hl}^{\frac{3}{2}}\frac{1}{z}} - \frac{8}{3}\,\frac{zz}{\mathrm{hl}^{\frac{1}{2}}\frac{1}{z}} + \frac{16}{3}\sqrt{\frac{\pi}{2}}.$$

iii. $\int_0^1 \frac{(1-z)\,\mathrm{d}z}{\mathrm{hl}^{\frac{3}{2}}\frac{1}{z}} = \int_0^1 \frac{\chi\mathrm{d}z}{\mathrm{hl}^{\frac{3}{2}}\frac{1}{z}} = \left(\frac{4}{\sqrt{2}} - 2\right)\sqrt{\pi} =$

$$(2\,(\sqrt{2}-1)\,\sqrt{\pi} = .828427\,\sqrt{\pi}.$$

iv. $\int_0^1 \frac{zz\mathrm{d}z}{\sqrt{\mathrm{hl}\,\frac{1}{z}}} = \sqrt{\frac{\pi}{3}}$; $\int_0^1 \frac{zz\mathrm{d}z}{\mathrm{hl}^{\frac{3}{2}}\frac{1}{z}} = 2\,\frac{z^3}{\mathrm{hl}^{\frac{1}{2}}\frac{1}{z}} - 6\sqrt{\frac{\pi}{3}}$;

$$\int_0^1 \frac{zz\mathrm{d}z}{\mathrm{hl}^{\frac{5}{2}}\frac{1}{z}} = \frac{2}{3}\,\frac{z^3}{\mathrm{hl}^{\frac{3}{2}}\frac{1}{z}} - 4\,\frac{z^3}{\mathrm{hl}^{\frac{1}{2}}\frac{1}{z}} + 12\sqrt{\frac{\pi}{3}}.$$

v. $\int_0^1 \frac{(1-z)^2\mathrm{d}z}{\mathrm{hl}^{\frac{5}{2}}\frac{1}{z}} = \left(\frac{-4}{3} + \frac{2.16}{\sqrt{2.3}} - \frac{36}{\sqrt{3.3}}\right)\sqrt{\pi} = .719064\,\sqrt{\pi}.$

vi. $\int_0^1 \frac{(1-z)^3\mathrm{d}z}{\mathrm{hl}^{\frac{7}{2}}\frac{1}{z}} = .642767\,\sqrt{\pi}.$

Now the value of $v = \sqrt{(x^2 - s^2 - 2p\chi s^2)}$ becomes here $\sqrt{(x^2 - 1 - 2p\chi)}$; and since $x - 1 = \int \mathrm{d}x = -\int \frac{\mathrm{d}y}{mz}$, making $\int \mathrm{d}x = \Xi$, or $x^2 = (\Xi + 1)^2$, we have $x^2 - 1 = \Xi^2 + 2\Xi$; but the actual extent of the atmosphere is so limited, that we may neglect Ξ^2, in comparison with 2Ξ, without sensible error, and make $x^2 - 1 = 2\Xi$, and $\mathrm{d}r = \frac{-p\mathrm{d}z}{\sqrt{(2\Xi - 2p\chi)}} = \frac{-p}{\sqrt{2}}\cdot\frac{\mathrm{d}z}{\sqrt{\Xi}}\left(1 - \frac{p\chi}{\Xi}\right)^{-\frac{1}{2}} = \frac{-p}{\sqrt{2}}\cdot\frac{\mathrm{d}z}{\sqrt{\Xi}}\left(1 + \frac{1}{2}\,\frac{p\chi}{\Xi} + \frac{3}{8}\,\frac{p^2\chi^2}{\Xi^2} + \frac{5}{16}\,\frac{p^3\chi^3}{\Xi^3} + \ldots\right) = \frac{-p}{\sqrt{2}}\left(\frac{\mathrm{d}z}{\sqrt{\Xi}} + \frac{1}{2}\,\frac{p\chi\mathrm{d}z}{\Xi^{\frac{3}{2}}} + \frac{3}{8}\,\frac{p^2\chi^2\mathrm{d}z}{\Xi^{\frac{5}{2}}} + \ldots\right)$. This expression is applicable to every hypothesis by which the relation of x to z can be expressed; and in the case of a uniform temperature, since $\mathrm{d}y = -mz\mathrm{d}x = \mathrm{d}z$, and $\mathrm{d}x = \frac{-\mathrm{d}z}{mz}$, we have $\Xi = -\int \frac{\mathrm{d}z}{mz} = \frac{1}{m}\,\mathrm{hl}\,\frac{1}{z}$, and $\mathrm{d}r = -p\sqrt{\frac{m}{2}}\left(\frac{\mathrm{d}z}{\sqrt{\mathrm{hl}\,\frac{1}{z}}} + \frac{1}{2}\,mp\,\frac{\chi\mathrm{d}z}{\mathrm{hl}^{\frac{3}{2}}\frac{1}{z}} + \frac{3}{8}\,m^2p^2\,\frac{\chi^2\mathrm{d}z}{\mathrm{hl}^{\frac{5}{2}}\frac{1}{z}} + \frac{5}{16}\,m^3p^3\,\frac{\chi^3\mathrm{d}z}{\mathrm{hl}^{\frac{7}{2}}\frac{1}{z}} + \ldots\right)$. Then, by the integration already

explained, $\int_0^1 \mathrm{d}r = -p\sqrt{\frac{m\pi}{2}}\,(1 + .414214\,mp + .269649\,m^2p^2 + .200865\,m^3p^3 + \ldots)$ or, taking $\frac{1}{p} = 3403$, and $m = 798$, $r = .010423\ (1 + .097133 + .014830 + .00259 + [.0005]) = .010423 \times 1.1151 = 39'\,57$; so that the former result was too great by 18″: and if we make $\frac{1}{p} = 3540$, and $m = 766$, we find $r = .0097988\ (1 + .08963 + .01263 + .00203 + [.00040]) = .0097988\ (1.1047) = .010825 =$ 37′ 13″, or 16″ less than the former computation made it; the difference, which before came out 2′ 46″, being now found, a little more accurately, 2′ 44′.

The relation between x and z may be computed from the hypothesis of an equable variation of temperature in ascending, according to the statement expressed by the equation $z = y\ (1 + tx - t)$ (*supra*, p. 36. D), or $z = yw$, whence

$\mathrm{d}y = \frac{\mathrm{d}z}{w} - \frac{z\mathrm{d}w}{ww}$, $\qquad \frac{\mathrm{d}z}{z} = \frac{\mathrm{d}w}{w} + \frac{w\mathrm{d}y}{z}$, but

$\mathrm{d}y = -mz\mathrm{d}x$, and $\qquad \frac{\mathrm{d}z}{z} = \frac{\mathrm{d}w}{w} - mw\mathrm{d}x$; consequently

$\mathrm{hl}z = \mathrm{hl}w - m\!\int\! w\mathrm{d}x$, and $\qquad z = we^{-m\int w\mathrm{d}x} = (1 + tx - t)\, e^{-m(x + \frac{1}{2}txx - tx)}$; and from this expression we may find the density z corresponding to any height x, upon the supposition that the bulk of a given quantity of air varies *proportionally* with a uniform variation of temperature, and not *uniformly*, as the experiments of Schmidt and Gay Lussac induced them to infer with respect to ordinary temperatures. (See Nat. Philos., Vol. II., p. 393.) If we computed the horizontal refraction from this equation by means of the series beginning with $\frac{\mathrm{d}z}{\Xi}$, we should have to substitute, for $\mathrm{d}z$, $(1 + tx - t)\ e^{-m(x + \frac{1}{2}txx - tx)}\ (-m\mathrm{d}x - mtx\mathrm{d}x)$ and for Ξ, $x - 1$.

Besides the equation $y = az^2 + bz^3 + cz^4 + \ldots$, there may probably be many others, not far from the true constitution of the atmosphere, which would afford finite expressions for the refraction; thus if $y = z^{\frac{3}{2}}$, we have $\mathrm{d}x = \frac{-\mathrm{d}y}{mz} = -\frac{3}{2}\frac{\sqrt{z}\,\mathrm{d}z}{mz}$ and $\Xi = x - 1 = \frac{3}{m}\,(1 - \sqrt{z})$, whence $\mathrm{d}r = \frac{-p\mathrm{d}z}{\sqrt{(2\Xi - 2p\chi)}} =$

$$\frac{-p\mathrm{d}z}{\sqrt{\left(\frac{6}{m}-\frac{6}{m}\sqrt{z}-1+z\right)}}, \text{ or, if } \sqrt{z}=\psi, \frac{-2p\psi\mathrm{d}\psi}{\sqrt{\left(\frac{6}{m}-1-\frac{6}{m}\psi+\psi^2\right)}},$$

and the fluxion assumes the form $\frac{x\mathrm{d}x}{\sqrt{(a+bx+cx^2)}}$, which is easily integrated, and there is little doubt that such a hypothesis, if advanced with sufficient pomp and ceremony, would be allowed to represent the constitution of the lower parts of the atmosphere, which are principally concerned in the refraction, much better than that of Bessel, though, perhaps, not quite so accurately as they might be represented by a more appropriate, though less convenient exponent.

London, 6th Aug., 1823.

This last hypothesis in a more extended form, making $y=\frac{3}{2}z^{\frac{3}{2}}-\frac{1}{2}z^2$, was further developed in a Memoir in the Philosophical Transactions for 1824, entitled 'A Finite and Exact Expression for the Refraction of an Atmosphere nearly resembling that of the Earth,' which is reprinted below, No. XXXI.—*Note by the Editor*.

No. XXXI.

A FINITE AND EXACT EXPRESSION FOR THE

REFRACTION OF AN ATMOSPHERE

NEARLY RESEMBLING THAT OF THE EARTH.*

From the Philosophical Transactions for 1824.

Read February 5, 1824.

It has lately been demonstrated, in the Journal of the Royal Institution, that if the pressure of the atmosphere y be represented either by the square or by the cube of the square root of the density z, the astronomical refraction r may be obtained in a finite equation. Mr. Ivory, in a very ingenious and elaborate paper lately presented to the Royal Society, has computed the refraction, by means of several refined transformations, and with the assistance of converging series, from an equation which expresses the pressure in terms of the density and of its squares: I have now to observe, that if we substitute, for the simple density, the cube of its square root, and make $y = \frac{3}{2} z^{\frac{3}{2}} - \frac{1}{2} z^2$, we shall represent the constitution of the most important part of the atmosphere with equal accuracy, although this expression supposes the total height somewhat smaller than the truth, and belongs to one of those hypotheses, which Mr. Ivory has considered as inadmissible: it has the advantage, however, of affording a direct equation for the refraction, which agrees very nearly with Mr. Ivory's table,

* Mr. Ivory, in the 'Philosophical Magazine' for 1825 (vol. lxv. p. 34), states that the solution which forms the subject of this Memoir is an immediate consequence of his general formula in the 'Philosophical Transactions' for 1823, and that he did not deduce it, because he conceived that the constitution of the atmosphere which the hypothesis in the text represented, did not approach so near to nature as others which he considered.—*Note by the Editor.*

and still more accurately with the French table and with that which has been published for some years in the Nautical Almanac.

Since $\mathrm{d}\,x = \frac{-\mathrm{d}\,y}{m\,z}$, m being the number of times that the modulus of the atmospherical elasticity is contained in the radius of the earth, and here $\mathrm{d}y = \frac{9}{4}\sqrt{z}\mathrm{d}z - z\mathrm{d}z$, we have $\mathrm{d}x = -\frac{9}{4}\frac{\mathrm{d}z}{m\sqrt{z}} + \frac{\mathrm{d}z}{m}$, and $\int \mathrm{d}x = -\frac{9}{2\,m}\sqrt{z} + \frac{z}{m} + \frac{7}{2\,m}$, for the height above the earth's surface, which, when $z = 0$, becomes $\frac{7}{2} \times 27300 = 95550$ feet. For the refraction, we have the equation $\mathrm{d}\,r = \frac{-p\,\mathrm{d}\,z}{\sqrt{(x^2 - s^2 - 2p[1-z])}}$ * $= \frac{-p\,\mathrm{d}\,z}{\sqrt{(2\int \mathrm{d}\,x + v^2 - 2\,p + 2\,p\,z}}$, which is the value originally assigned to this fluxion by Dr. Brook Taylor; v being the sine of the apparent altitude; and here $\mathrm{d}r = \frac{-p\,\mathrm{d}\,z}{\sqrt{\left(\frac{7}{m} - \frac{9}{m}\sqrt{z} + \frac{2\,z}{m} + v^2 - 2\,p + 2pz\right)}}$; or, if $\sqrt{z} = \psi$, and $\mathrm{d}\,z = 2\,\psi\,\mathrm{d}\,\psi$, $-\frac{\mathrm{d}\,r}{2\,p} = \frac{\psi\,\mathrm{d}\,\psi}{\sqrt{\left(\frac{7}{m} - 2\,p + v^2 - \frac{9}{m}\psi + \left[\frac{2}{m} + 2\,p\right]\psi^2\right)}}$ which is equivalent to the fluxion $\frac{x\,\mathrm{d}\,x}{\sqrt{(a + b\,x + c\,x\,x)}}$ of the Article Fluents in the Encyclopædia Britannica, No. 259;† the fluent being $\frac{1}{c}\left[\sqrt{(a + b\,x + c\,x^2)} - \frac{b}{2\sqrt{c}}\,\mathrm{h\,l}\,(2\,c\,x + b + 2\sqrt{c}\sqrt{[a + b\,x + c\,x^2]})\right]$, and its whole value, from $z = 1$ to $z = 0$, being $-\frac{c\,r}{2\,p} = \sqrt{(a' + v^2)} - v + \frac{b}{2\sqrt{c}}\,\mathrm{h\,l}\,\frac{2\,c + b + 2\,v\sqrt{c}}{b + 2\sqrt{c}\sqrt{(a' + v\,v)}}$, putting $a' = \frac{7}{m} - 2\,p$, since $a + b + c = v^2$.

For the numerical values of the coefficients, taking, at the temperature of 50°, $p = .0002835$, and $\frac{1}{m} = .001294 = \frac{1}{772.8}$, $a = \frac{7}{m} - 2p + v^2 = .008491 + v^2$, $b = \frac{-9}{m} = -.011646$, and $c = \frac{2}{m} + 2\,p = .003155$; hence $\frac{2\,p}{c} = .17972$, $\sqrt{a'} = .0921466$, $\sqrt{c} = .05617$, $\frac{-b}{2\sqrt{c}} = .10367$, $2\,c + b = -.005336$, and $r = .17972$

* Supra, No. XXX., p. 40. † See also Hirsch's 'Integraltafeln,' lxiv.

$\left(\sqrt{(.008491+v^2)}-v-.10367\,\mathrm{h\,l}\,\frac{.005336-.11234\,v}{.011646-.11234\sqrt{(.008491+vv)}}\right)$;
and at the horizon, when $v=0$, $r=.17972\left(.0921466-.10367\,\mathrm{h\,l}\,\frac{.005336}{.011646-.11234\times.092147}\right)=.009840=33'\,42'',5$; which is only 1'',5 less than the quantity assigned by the French tables and in the Nautical Almanac, while Mr. Ivory makes it 34' 07'',5. Again, if we take $v=.1$, for the altitude 5° 44' 21'', we obtain 8' 49'',5 for the refraction, while the Nautical Almanac gives us 8' 53'', and Mr. Ivory's table 8' 49'',6. There is, however, no reason for proceeding to compute a new table by this formula, the method employed for the table in the Nautical Almanac being rather more compendious in all common cases: and even if it were desired to represent Mr. Ivory's table by the approximation there employed, we might obtain the same results, with an error never much exceeding a single second, from the equation $.00028333=\frac{v}{s}\,r+\frac{2.26+\frac{1}{2}vv}{ss}\,r^2+5400\,\frac{rr}{ss}\left(\frac{v}{s}\,r+\frac{1.13+\frac{1}{2}vv}{ss}\,r^2\right)$.

Weibeck Street, 3rd February, 1824.

No. XXXII.

HISTORICAL SKETCH OF THE VARIOUS SOLUTIONS
OF THE PROBLEM OF

ATMOSPHERICAL REFRACTION.

FROM THE TIME OF DR. BROOK TAYLOR TO THAT OF THE LATEST COMPUTATIONS.

From Brande's Quarterly Journal for 1825, vol. xviii. p. 347.

IN justice to the claims of departed merit, it is often necessary to revert to the first steps by which the inventions of modern mathematicians have been prepared, if not anticipated: but it seldom happens that the earlier solutions of a problem have been so completely forgotten as appears to be the case with the investigations of Dr. Brook Taylor respecting the path of light in the earth's atmosphere. Besides having apparently furnished to Newton an instrument with which he has dazzled the admiring gaze of some later philosophers, they exhibit also a remarkable specimen of a mode of analysis which seems to afford a general if not a universal rule for the integration of any given fluxion by induction: that is, to find a series of successive *fluxions* of the given fluxional quantity, and to express the relation of each term of this series to the preceding one in a general formula, which, being applied to the first term, or to the fluxion itself, must naturally afford us the *fluent* required.

Part I. BROOK TAYLOR, NEWTON, SIMPSON, KRAMP, *and* LAPLACE.

[The translation (with occasional comments interposed) of the 27th and last Proposition of Taylor's 'Methodus Incrementorum,' which is given in the original article, is omitted.*]

* It appears from several letters of Newton addressed to Flamsteed, in the course of the years 1694 and 1695, that the subject of Refraction had long occupied his

Account of SIR ISAAC NEWTON'S *Table.*

Dr. Halley, in the 'Philosophical Transactions' for 1721, has published a Table of Refractions, which he says was "the first accurate table" made by the "worthy President of the Society:" "the curve which a beam of light describes, as it approaches the earth, being one of the most perplexed and intricate that can well be proposed, as Dr. Brook Taylor, in the last Proposition of his 'Methodus Incrementorum,' has made it evident. The aforementioned Table, I here subjoin for the use of the curious, such as I *long since* received it from its great author; it having never yet, that I know of, been made public." The refraction at the horizon is made 33′ 45″; at 45°, 54″ only, agreeing certainly in the latter case with Hawkesbee's experiments mentioned by Taylor. From the way in which Taylor's investigations are mentioned by Halley, it might naturally be supposed that Newton's computations were independent of Taylor's formulæ; and hence it was natural enough, that Professor Kramp should spare himself the labour of consulting Taylor's book, which has by no means been generally known, though there is no doubt that the table might be computed from some of Taylor's different series: and even if the horizontal refraction were wanting, it might be obtained from the five neighbouring results, by making the fourth difference constant. After this explanation, it will still be interesting to observe the view which Kramp has taken of the history of the

attention. He first investigated the curve of Refraction upon the supposition that the density decreases uniformly; a supposition which he subsequently recognizes as untenable, inasmuch as it would make the refracting power of the atmosphere as great at the top as at the bottom. He afterwards appears, however, to have investigated the curve, and therefore the quantity of the refraction upon the same assumptions respecting its constitution, as are made in the 22nd Proposition of the 2nd book of the Principia, which assumes the temperature uniform throughout its extent. It was upon this hypothĕsis that his Tables of Refraction were deduced, which were communicated to Flamsteed in 1695, and which Halley published in 1721. In this remarkable correspondence Newton first distinctly pointed out the influence of the barometer and thermometer upon the amount of the refraction, and showed that the chief effects of vapours were secondary only, as modifying the temperature, and therefore the density of the air. The existence of those letters, which Mr. Baily first published in 1835, was unknown to Dr. Young, and he was mistaken therefore in considering Brook Taylor as the first philosopher who had dealt with this difficult problem: the 'Methodus Incrementorum' was not published before 1716, more than twenty years after the researches of Newton.—*Note by the Editor.*

problem; for though it will scarcely be practicable to claim for Taylor the whole of the merit which Kramp attributes *upon suspicion* to Newton, yet certainly much may be learned from Taylor's method of conducting the process.

Observations on Newton's Table, by KRAMP. Analyse des Réfractions Astronomiques et Terrestres. 4. Strasb. 1798.

"I. 59. Let us take, for a last example, the table of refractions left us by the greatest of all mathematicians, and the ablest of all observers, that have ever existed; the man, without whose discoveries our astronomy would scarcely deserve the name of a science, since it is to him alone that we are indebted for our knowledge of the eternal laws of nature, and for the application of computation to these laws; in a word, by the immortal NEWTON. Some years before his death, he communicated to his friend Halley the Table of Refractions, which the latter *eagerly* published [*s'empressa*] in the 'Philosophical Transactions' for 1721. We are not informed *how* this table was constructed, nor if it is the result of analysis or of observation; if the former, it would be interesting to have the mode of computation employed by Newton. He would have done better undoubtedly if he had explained it: but he was at that time in his eightieth year: let us respect his old age, and let us accept the table such as it is, with the gratitude due to its author.

"60. The Table of Newton gives 33′ 45″ at the horizon, and 54″ at 45°: hence the index of refraction is .0002618 [Hawkesbee's .00026414]. corresponding to a temperature of 74° of Fahrenheit. And on the other hand, at the temperature of 74°, the horizontal refraction of Newton is exactly what it ought to be, supposing the temperature of the atmosphere uniform throughout. Now if this agreement depended on direct observation, it would perhaps be a case unparalleled in the whole history of the physical sciences, especially as we shall see hereafter, that all the refractions in the neighbourhood of the horizon, agree almost as exactly with the conditions of the analysis, which they are very far from doing in the three tables of Bouguer. If the table was calculated, we may first ask,"

says Kramp, "for what reason Newton fixed on the temperature of 74° rather than any other:" but in fact he fixed on no temperature: "and secondly, how he arrived at the formula, which alone is capable of making the refraction exactly 33′ 45″ at this temperature; and this difficulty is not easily removed: for in fact, that formula depends on a very refined investigation, which was unknown to Euler in 1754, and of which the principles were not well explained before the publication of the Essay of Laplace, 'On the Approximation of Formulæ containing Factors raised to High Powers,' in the Memoirs of the Academy of Paris for 1782. Are we to suppose that the great Newton obtained the same conclusions at the beginning of the century, by modes of reasoning which he has left unexplained? This is not, indeed, absolutely impossible for a mathematician to whom nothing was impossible in the higher analysis; but it would be singular that he should have left no trace of the discovery in any other of his immortal writings." So elegant and so important a demonstration could certainly not have been left unrecorded by Newton, if it had occurred to him: but he does not appear to have entered, in the latest period of his life, into any very deep speculations relating to pure mathematics; nor to have been employed on any physical problem which was likely to lead him to the investigation, having in all probability found it sufficient for the present purpose to follow the steps of Taylor's ingenious researches.

Theory of SIMPSON *and Table of Bradley.*

The greatest practical improvement on Newton's Table was made before the year 1743 by Thomas Simpson, who computed the effects of the atmosphere on a ray of light, upon the supposition of a uniform decrease of the air's refractive force in ascending, and obtained from observations communicated to him by Dr. Bevis, a table which gives 33′ 0″ for the horizontal refraction, and 53″ for the altitude 45°. He computes his refractions by taking $\frac{1}{11}$ of the difference of two arcs of which the sines are as 1 to .9986: and he observes, that the distribution of heat in the atmosphere is the reason why the computation

upon the supposition of an equable temperature is so erroneous; though he exaggerated the error of this hypothesis so much, as to make the horizontal refraction 52′: being probably unacquainted with the computations of Taylor and Newton, which had been published twenty or thirty years before.

The Table of Bradley, which has been so universally admired and employed by the English astronomers and navigators, was obtained from that of Simpson, by the very slight modification of adopting $\frac{1}{6}$ instead of $\frac{2}{11}$ for the multiplier of the difference of the arcs, the correction having of course been obtained from observation only; but with the fondness for approximative computation which has often been remarked in practical astronomers, Dr. Bradley chose to begin by supposing the approximate refraction known, and to correct it so as to make it exactly proportional to the tangent of the zenith distance diminished by three times the refraction. Leaving the horizontal refraction 33′ 0″, as assigned in Simpson's Table, he makes it 57″ at 45°, instead of 53″. Dr. Bradley has, however, the merit of having first introduced an accurate mode of allowing for the effect of the actual temperature of the atmosphere at the place of observation, which he estimates at $\frac{1}{400}$ of the whole refraction for each degree of Fahrenheit above or below the standard temperature of 50°.

Account of Euler's *Investigations, from Kramp*, Chap. v.

The Memoir of Euler, contained in the Transactions of the Academy of Berlin for 1759, though of no importance whatever to optics or to astronomy, may however become still more useful, if properly considered, in a moral point of view, than if it had been completely successful. It may not only teach us a proper diffidence in our own computations, but it may serve to show, among many other instances, how liable the greatest and wisest of mankind are to imperfections and errors, even in those departments which they have cultivated at other times with the greatest success. An extract from the account given by Kramp of Euler's results, will render it sufficiently obvious, how much valuable time and useless labour might have been spared if

Euler had only happened to look at a few pages of Taylor's little work, which was printed nearly fifty years before.

The formula adopted by Euler for expressing the elasticity E at the height x is $E = \frac{f}{f+x}$, f being the subtangent or modulus, which differs very little from the logarithmic progression of densities when the temperature is supposed constant, that is, $E = e^{-x:g}$. From this expression he deduces the fluxion of the angle described by the ray; but in attempting to assign its fluent, the art of this profound mathematician has completely failed him; and he has thought it justifiable to have recourse to the very arbitrary and incorrect supposition that the curve may be considered as nearly agreeing with a hyperbola, having for its equation $t = Cy^{m-1}$. The refraction is indeed easily computed upon this hypothesis; but it becomes, as Kramp has shown, at the horizon, 42′, instead of 36′ 26″ as it ought to be in the supposed state of the atmosphere, and at 45°, no less than 2½′ instead of about 57″. In short, the failure could not possibly have been more total, if the essay had been the work of the idlest schoolboy, instead of one of the four or five greatest mathematicians that have ever existed; for in the same rank with Archimedes and Newton, and Euler and Laplace, it is difficult to say what fifth philosopher has any right to be classed: perhaps Leibnitz, and possibly Lagrange; but this question will long remain undecided, if it requires to be determined by a jury of their peers.

Methods of Mayer *and* Lambert. Kramp, v. 41, 47.

The formula of Mayer was published without demonstration in his Lunar Tables, and appears to have been only an empirical modification of those of Euler and Bradley, approaching so nearly to Bradley's results as scarcely to require any distinct consideration.*

Lambert published in 1759, at the Hague, a separate essay,

* The opinion of Bessel respecting Mayer's merits differs greatly from that of Dr. Young: after referring to the nearly contemporary labours of Bradley on this subject, he adds *cæterum in hoc capite non æquales solum verum etiam posteriores astronomos antecessit Tobias Mayer, in refractionis formula rectius adhibens thermometri correctionem.* 'Fundamenta Astronomiæ,' p. 52.—*Note by the Editor.*

entitled *Les propriétés remarquables de la route de la Lumière par les airs;* and he resumed the subject in the Berlin Almanac for 1779. He has adopted in this research a supposition respecting the asymptote of the curve which Mr. Kramp has shown to be inadmissible; and his method of computation exhibits an error at the horizon amounting to 87″. But "Lambert might have concluded," says Kramp, "from his own geometrical investigations, the approximate result which Mayer had already obtained in part, that is, that for the same absolute elasticity" as expressed by the height of the barometer, "the horizontal refraction must be reciprocally proportional to the square root of the cube of the specific elasticity," depending on the temperature; and he contradicted himself when he objected to what Mayer had said on this subject.

Epoch of Kramp *and* Laplace.

For the mathematical theory of refraction it may be said that nothing of immediate importance was done from the time of Newton and Taylor, to that of Laplace and Kramp. It is true, that the XVIth volume of the New Commentaries of Petersburg, for the year 1771, contains an Essay of Euler, in which the particular value of a fluent is first demonstrated, which is of singular importance in abridging the computation of the horizontal refraction; but it does not seem to have occurred to this great mathematician in what manner his discovery might be rendered serviceable for the solution of a physical problem. It was in the Memoirs of the Academy for 1782, that *Laplace* made public an essay on the integration of differential functions, which contain very high powers of their factors; and this essay Kramp considers as first developing the principle that led to the more accurate solution of the problem.

Professor Kramp had made himself known and respected in the mathematical world, by his attempts to apply the principles of mechanical hydraulics to the circulation of the blood in health and in disease, and he was the author of some interesting essays on the combinatorial analysis of Hindenburg, which excited at one period so much attention in Germany, though none

of its other results appear to have been so satisfactory as those which are contained in the chapter on Numerical Faculties of the Analyse des Réfractions. The rapid and brilliant progress that is displayed in this chapter through some of the most thorny paths of analysis will for ever distinguish its author among the original contributors to the advancement of methematical analysis; but it is, perhaps, somewhat too rapid to have avoided all traces of contact with the thorns that were to be encountered. The originality consists principally in the very great generalisation of the laws of the faculties of numbers, which have been since more commonly called factorials, and in their extension to faculties with fractional indices, formed according to the analogy of fractional powers, but which, in fact, though they may be shown to have real values, are little less imaginary in their immediate structure, than the square roots of negative numbers, and resemble still more nearly the fluxions of fractional orders. Having first deduced from the series which expresses the relation of the sides of any two polygons, the general value of the product of two faculties (§ 16): he transforms the faculty $1^{m:n-1}$, divided by m, into another which he shows to agree in its general term with the series expressing the fluent ${}^{0}\!\int_{\infty} t^{m-1} e^{t^n} dt$, as it is obtained from the series of Taylor, or of Bernoulli, for integration by parts. He derives also, from a similar method of investigation, some very compendious expressions for computing the same fluent for any other values of t, and gives, at the end of his volume, some tables of their results, which have lately been much extended by Bessel in his Fundamenta. For the horizontal refraction, which is expressed by $\surd\,(\tfrac{1}{2}n\omega\pi)\,(1 + An + Bn^2 + \ldots)$ he finds $A = .414214$, $B = .262649$, $C = .200865$, $D = .160253$, and $E = .132935$ (see No. XXX.); and from the values assigned by Laplace in his exposition, he computes the refraction equal to 7307 decimal seconds at the freezing temperature, differing but little from the 7300 assigned to it by Laplace. Concluding from observation, that a uniform temperature of the atmosphere will not properly represent the actual refractions, he suggests the alteration of the quantity denoting the subtangent, or modulus of elasticity, in such a manner as to correspond with the actual

state of the phenomena; and this is precisely what has since been attempted by Professor Bessel. He also observes, that the refractions near the horizon by no means follow the exact proportion of the densities, and gives a table extending from 10° to 100° of Fahrenheit, which shows that, within these limits, the refraction varies in the ratio of 27 to 37, while the densities are supposed to vary only in the proportion of 21 to 25 or 52 to 62.

In the precise determination of the refractions very near the horizon, Professor Kramp has not been particularly fortunate. The terrestrial refraction, which is the subject of his fifth chapter, presents no particular difficulty; and neither of these investigations, as belonging to the hypothesis of an equable temperature, presents any remarkahle interest at present. The method of Laplace, which is well known from the Mécanique Céleste, has deservedly superseded that of Kramp, especially from the extreme elegance and conciseness with which the definite fluent already mentioned is there obtained by means of the method of partial fluxions; and the hypothesis respecting the distribution of temperature, which has been practically adopted by this illustrious philosopher, has led to the construction of tables possessed of accuracy abundantly sufficient for every purpose of astronomy, and which ought never to have been set aside by the German astronomers, in order to return to the mere speculative suggestion of Kramp, however elaborately computed and partially supported by their ingenious countryman, Professor Bessel. At this period of the history of refraction, the investigation had attained all the practical perfection that could be desired: it will be proper to proceed in the second place to the consideration of the later attempts that have been made to improve it by the mathematicians and astronomers of the British empire.

Part II. *Account of the later improvements in the theory of* Atmospherical Refraction.

From the time of the publication of the French tables of refraction, constructed from the computations of the illustrious Laplace, the determination has acquired a degree of accuracy rather exceeding than falling short of what might have been

expected from the fluctuating state of the elements on which it depends.

Our countryman, Mr. Groombridge, is the first astronomer that seems to have undertaken an elaborate series of observations almost entirely for the purpose of obtaining a complete table of refractions. His first publication is contained in the Philosophical Transaction for 1810. The mode of computation that he has employed, to obtain the mean refraction at a given altitude, is to observe the same star above and below the pole; and to make the sum or difference of the apparent altitudes, which, compared with the double latitude, gives the sum or difference of the refractions at the given altitudes: then by comparing these results with those of an approximate table, he finds the mean factor required for multiplying the numbers of the table; and in this manner he has obtained for Bradley's Table, first reduced on account of the sun's parallax, the factor 1.02845, and has proposed still further to improve it by adopting the form

$$r = 58.1192 \tan (Z.\,.D. - 3.3625r).$$

Mr. Groombridge has not recorded the particular temperatures of his observations, but has reduced them to the mean temperature of the table, which is supposed to be 49° for the interior thermometer, and 45° for the exterior. The results may, however, be of use in continuing upwards the Empirical Table, inserted in a former number of this Journal (Vol. XV.) from Mr. Groombridge's later observations, and it will be perfectly justifiable to divide the sum of the two refractions in the ratio of the corresponding refractions of any approximate table, in order to determine the larger of the two with little chance of error. In this manner we may obtain the following Table, selecting the observations, at convenient altitudes, which have been most frequently repeated.

Stars.	Obs.	Alt.	Refr.	N. A.		Diff.
		° ′ ″	′ ″	′ ″		″
α Pers.	12	10 42 41.0	4 35.2	4 58.7	+	3.5
β Cast.	7	15 38 37.0	3 23.9	3 25.5	+	1.6
2 Lync.	7	20 34 14.6	2 31.4	2 34.0	+	2.6
α Ceph.	5	28 33 11.9	1 45.9	1 47.1	+	1.2
5 Urs. Min.	5	38 2 32.3	1 14.7	1 14.3	−	0.4
Camelop. H. 30	9	45 0 46.5	0 57.5	0 58 1	+	0.6
Polaris	41	49 45 31.5	0 48.7	0 49.2	+	0.5

Mr. Groombridge has also taken some pains to ascertain from observation, the magnitude of the thermometrical correction, though without distinguishing the different effects at different altitudes: and he finds for the exterior thermometer .0021 for every degree of variation reckoned from 45°, and .0023 or .0024 for the degrees of the interior thermometer reckoned from 49°. His own formula is thus compared with the French Tables, and with Piazzi's empirical correction. Barometer 29.6.

Alt.	Gr. 1810.	Fr. T.	Piazzi.	Gr. 1814.
° ′	′ ″	′ ″	′ ″	′ ″
0 0	31 27.9	33 46.3	32 3.0	34 28.1
1 0	23 46.8	24 21.2	23 46.1	24 32.9
2 0	18 19.2	18 22.2	18 2.7	18 19.8
2 0	14 31.7	14 28.1	14 25.1	14 19.8
4 0	11 52.2	11 48.3	11 42.6	11 45.3
5 0	9 57.3	9 54.3	9 45.4	9 53.0
10 0	5 19.8	5 19.8	5 16.1	5 19.2
20 0	2 38.4	2 38.8	2 37.8	2 38.3
45 0	0 58.0	0 58.2	0 57.2	0 58.0

We find in the Transactions for 1814 a continuation of Mr. Groombridge's researches extended to the refraction of stars near the horizon. He observed, that the results, corrected according to the indications of the thermometer *without*, are the most correct; he alters the thermometrical factor from .0021 to .002, and adopts finally the expresion $r = 58.'' 123976 \times \tan (Z.D - 3.634295r)$, reducing it, below 87°, .00462 for each minute.

Dr. Brinkley, in 1810, had acquiesced in the formula of Simpson and Bradley, with a slight modification, and with the French correction for temperature, that is $r = 56''.9 \tan (Z.D - 3.2r) \frac{B}{29.6} \cdot \frac{500}{450 + F}$. (Ph. Tr. p. 204.)

Dr. Brinkley has pursued the subject with his accustomed accuracy in the Irish Transactions for 1815, and has employed 65 observations of Capella and 42 of Lyra, together with a multitude of others, confirming the accuracy of the French Tables. He has shown the agreement of several assumed hypotheses, in moderate zenith distances. Thus, at 50° F. and 29.6 B.: —

Alt.	Obs.	Fr. T.
°	′ ″	′ ″
16	3 18.6	3 18.2
20	2 37.3	2 37.0
30	1 39.7	1 39.4
40	1 8.7	1 8.6
45	0 57.7	0 57.6

He therefore recommends the employment of the French Tables for moderate zenith distances, and remarks that nearer the horizon it is useless to expect minute accuracy in any conclusion from astronomical observations.

In the XIIth volume of the Irish Transactions, we find a memoir of Dr. Brinkley, read in 1814,* on the thermometrical correction of refraction; giving a method of correction "derived from the formula" of Simpson, "obtained in the hypothesis of a density decreasing uniformly." The author observes that "at present we have not sufficient observations to determine, whether the actual variations of refractions at low altitudes are most conformable to the theory of Mr. Bessel, to that of Dr. Young," or to his own: which, indeed, differ less from each other than they do from the corrections employed by the French, by Groombridge, or by Bradley; and after all, it seems impossible to expect much advantage from any theory in applying this correction to the accidental variations of any one climate, though it may very probably be of use for finding the mean refractions in distant latitudes. (See Vol. XV. of this Journal.)

It was in the interval between the publication of Dr. Brinkley's two papers, that Dr. Young annexed a new Table of Refractions to the 'Nautical Almanac,' founded on an approximation of his own, but agreeing almost exactly in the mean refractions with the French Tables, adopting, however, a correction for temperature derived from theory, and greater near the horizon than that which the French have employed. Having observed that the series obtained for expressing the refraction in terms of the density failed at the horizon, because the altitude was a divisor of the coefficients, it occurred to him that this inconvenience might be avoided, by expressing the density in a series of the powers of the refraction; and the formula

* See a notice of it in 'Brande's Quarterly Journal' for 1821, Vol. XI., p. 364. *Note by the Editor.*

thus obtained, though not always convenient in extreme cases, is still very useful for obtaining a tolerably accurate result with great facility from any imaginable theory, and is also capable of representing, by a few of its first terms only, with their coefficients empirically modified, the refraction either actually observed, or correctly computed, upon any possible hypothesis respecting the constitution of the atmosphere.

Dr Young's theorem is $p = \frac{v}{s} r + \left(\frac{\zeta}{mp} - 1\right) \frac{rr}{2ss} + \mathrm{d}\, \frac{\zeta}{mpz} \frac{r^3}{6s^3 \mathrm{d}r} + \ldots.$ When r vanishes, or near the zenith, the first term of the series only determines it, and it becomes simply proportional to the refractive density p; at a greater distance the second term become sensible, depending on the total variation of the actual density in ascending a given height, ζ being $= \frac{dy}{dz}$: this coefficient ought, therefore, to be the same in every hypothesis concerning the constitution of the atmosphere, which professes to represent correctly the initial diminution of temperature of density in ascending; how this diminution may vary at greater heights cannot easily be determined from direct observation, since we cannot reason with certainty on the temperature of the open atmosphere remote from the earth, from that of the surfaces of mountains, which may very possibly be affected by their immediate contact with the solid earth, and it seems necessary to obtain the subsequent coefficients from the phenomena of refraction, as observed in favourable circumstances, taking also the mean of a great number of results.

In the approximatory method of using four terms only, it may become convenient to modify even the first two, in order to co-operate the more perfectly with the succeeding ones; but it is difficult to suppose that the actual constitution of the atmosphere can be represented with *great precision* by a hypothesis like that of Laplace, in which the initial variation of temperature is made greater than the truth. That there is no actual necessity for such a departure from observation, is shown by Mr. Ivory's table, and by Dr. Young's latest solution of the problem, both of which begin with assuming the initial variation of temperature equal to that which is actually observed: while Mr. Ivory supposes the rate of variation to become slower

in ascending, and Dr. Young more rapid, and yet the results agree very nearly with each other, and with the French tables, except quite close to the horizon.

The ridiculous accusations which were brought against Dr. Young, and against the British Government, by an unfortunate enthusiast,* whose imprudence seems almost to have impaired his reason, might perhaps serve in some slight degree, if they served for anything, to make it probable that the method which he so clamorously professed to have improved, was in itself of *some* value: but as even this does not yet appear to be *universally* admitted, it may not be superfluous to give one or two additional instances of its application.

The general equation, as investigated in the fifteenth volume of this Journal,† is $ps = vr + \left(\frac{\zeta}{2mps} - \frac{s}{2}\right) r^2 + \left(\frac{\zeta'}{6mps} + \frac{\zeta v}{6mp^2s^2}\right) r^3 + \left(\frac{\zeta''}{24mps} + \frac{\zeta' v}{24mp^2s^2} + \frac{\zeta}{24mp^2s^2}\left(\frac{\zeta}{mps} - s\right) + \frac{\zeta v^2}{12mp^3s^3}\right) r^4 + \ldots;$ ζ being $= \frac{dy}{dz}$, $\zeta' = \frac{d\zeta}{dr}$, and $\zeta'' = \frac{d\zeta'}{dr}$; and we may take for $\frac{1}{m}$ .001294, and for p, .0002835, so that $\frac{1}{mp} = 4.5644$, and $\frac{1}{mp^2} = 16100$.

A. The first hypothesis of Kramp has been abandoned by Bessel on account of its intricacy, and it has lately been declared even by Mr. Ivory "*too complicated for calculation.*" We have here $z = e^{-\frac{1}{\iota}(e^{\iota\sigma}-1)+\iota\sigma}$, σ being $= m\ (x-1)$, and $d\sigma = m dx = \frac{-dy}{z}$, and since $\frac{dy}{dr} = \frac{-\zeta v}{ps}$, $\frac{d\sigma}{dr} = \frac{\zeta v}{psz}$: but $dz = z d\left(\frac{1}{\iota} - \frac{1}{\iota} e^{\iota\sigma} + \varepsilon\sigma\right) = z\varepsilon d\sigma\left(1 - \frac{1}{\iota} e^{\iota\sigma}\right) = -z\varepsilon\frac{dy}{z}\left(1 - \frac{1}{\iota} e^{\iota\sigma}\right) = -\varepsilon dy\left(1 - \frac{1}{\iota} e^{\iota\sigma}\right)$, and $\zeta = \frac{-1}{\iota - e^{\iota\sigma}}$, or initially $= \frac{-1}{\iota - 1}$, which must be $= \frac{5}{4}$, and $\iota = \frac{1}{5}$, and in general $\zeta = \frac{5}{5e^{\iota\sigma} - 1}$, whence $d\zeta = \frac{-25\iota e^{\iota\sigma} d\sigma}{(5e^{\iota\sigma}-1)^2}$, and $\frac{d\zeta}{dr} = \zeta' = \frac{-25\iota e^{\iota\sigma}\zeta v}{(5e^{\iota\sigma}-1)^2 psz}$, and lastly $\frac{d\zeta'}{dr} = \zeta'' = \zeta'\left(\frac{dv}{vdr} + \frac{de^{\iota\sigma}}{e^{\iota\sigma}dr} + \frac{d\zeta}{\zeta dr} - \frac{d(5e^{\iota\sigma}-1)^2}{(5e^{\iota\sigma}-)^2 dr} - \frac{dz}{zdr}\right)$: whence initially

* Hoene de Wronski.

† Supra, No. XXIX. p. 30.

$\zeta' = \frac{-25}{64}\frac{v}{ps}$, and $\zeta'' = \frac{-25}{64ps}\left(\frac{\zeta}{mps} - s + \frac{v\iota\zeta v}{ps} + \frac{\zeta' v}{\zeta} - \frac{8 d s v}{16 dr} + \frac{v^2}{ps}\right) = \frac{-25}{64ps}\left(\frac{\zeta}{mps} - s + \frac{\iota\zeta v^2}{ps} + \frac{\zeta' v}{\zeta} - \frac{\zeta v^2}{2ps} + \frac{v^2}{ps}\right) = \frac{-25}{64ps}\left(\frac{\zeta}{mps} - s + \frac{v^2}{4ps} - \frac{5v^2}{16ps} - \frac{5v^2}{8ps} + \frac{v^2}{ps}\right) = \frac{-25}{64ps}\left(\frac{\zeta}{mps} - s + \frac{5v^2}{16ps}\right)$. By substituting these values, we obtain the equation $ps = vr + \left(\frac{2.85275}{s} - \frac{1}{2}s\right) r^2 + 2306\frac{v}{ss} r^3 + \frac{576}{ss}\left(\frac{5.7055}{s} - s + 5450\frac{v^2}{s}\right) r^4$.

With this formula, we may proceed to calculate the refraction for the case $v = .1$, that is, for an altitude of 5° 44′ 21″, which will be allowed to be as low as can possibly be required for any accurate observations. Assuming then $r = .0026$, we have $r^2 = .000\,006\,76$, $r^3 = .000\,000\,017\,576$, and $r^4 = .000\,000\,000\,0457$; also $s = .99500$, $s^2 = .9900$, and $s^3 = .9850$, and the equation becomes $p = .00026130 + .00001610 + .00000411 + .00000157 + [.000\,001\,50] = .000\,28458$. so that .0026 is too much by about $\frac{1}{300}$, and $r = .00259$, which may be called certain to the last figure, giving 8′ 54″ .2, a result probably very near the truth in the actual mean state of the atmosphere.

B. Mr. Ivory's tables are constructed upon the hypothesis $y = .75z + .25z^2$; hence $\zeta = \frac{dy}{dz} = .75 + .5z$, $\zeta' = \frac{d\zeta}{dr} = \frac{.5dz}{dr} = \frac{-v}{2ps}$, and $\zeta'' = \frac{-1}{2ps}\frac{dv}{dr}$: the equation then becomes $p = \frac{vr}{s} + (2.85275 - \frac{1}{2}ss)\frac{rr}{ss} + 2012\,v\frac{r^3}{s^3} + 503\,(4.7055 + 7055\,v^2)\frac{r^4}{s^4}$, and assuming again $\frac{r}{s} = .0026130$, $\frac{r^2}{s^2} = .000\,006\,8276$, $\frac{r^3}{s^3} = .000\,000\,01784$, and $\frac{r^4}{s^4} = .000\,000\,000\,0467$, we have $p = .000\,261\,30 + .000\,016\,10 + .00000359 + .000\,00\,177 + [000\,002\,40] = .00028516$, too much by about .00000164, and we must subtract $\frac{1}{180}$, and we have $r = .002586 = 8' \, 53'' .4$, with an uncertainty that cannot exceed a few seconds: Mr. Ivory's table, which may possibly be correct, but which would naturally be a little within the truth rather than beyond it, since it is computed by a direct converging series, has 8′ 48″.0, which is 5″.4 less. It cannot be supposed that Mr. Ivory's method requires any such confirmation, but it would be easy to add a few more

terms to this series as a test, if there were any necessity for the perfect accuracy of the determination by two opposite methods.

C. The approximation lately communicated to the Royal Society,* which supposes $y = 1.5z^{1.5} - .5z^2$, gives $\zeta = \frac{\mathrm{d}y}{\mathrm{d}z} = 2.25\sqrt{z} - z$, whence $\frac{\mathrm{d}v}{\mathrm{d}r} = \frac{\zeta}{mpsz} - s = \frac{2.25}{mps\sqrt{z}} - \frac{1}{mps}$, $\mathrm{d}\frac{\mathrm{d}v}{\mathrm{d}r} = \frac{-1.125\mathrm{d}z}{mpsz\sqrt{z}}$, $\frac{\mathrm{d}^2v}{\mathrm{d}r^2} = \frac{1.125v}{mp^2s^2z\sqrt{z}}$, the fluxion of this initially $\frac{1.125}{mp^2s^2}\mathrm{d}v - \frac{1.6875v\mathrm{d}z}{mp^2s^2}\mathrm{d}z$, and $\frac{\mathrm{d}^3v}{\mathrm{d}r^3} = \frac{1.125}{mp^2s^2}\frac{\mathrm{d}v}{\mathrm{d}r} + \frac{1.6875}{mp^2s^2} \cdot \frac{v^2}{ps}$: consequently $p = \frac{v}{s}r + (2.85275 - \frac{1}{2}ss)\frac{rr}{ss} + 3019\,v\frac{r^3}{s^3} + 755\,(4.7055 + 5291\,v^2)\frac{r^4}{s^4}$: and, for $r = .0026$, $p = .0026130 + .000\,016\,10 + .000\,005\,39 + .000\,002\,03 + [.000\,002\,00.] = .000\,286\,82$, requiring for r a reduction of $\frac{1}{110}$, whence $r = 0025740 = 8' 50''.9$, with an uncertainty not exceeding 2″ at the utmost. And the direct computation by logarithms gives 8′ 49″ .6, differing only 1″ .3 from the series in this almost extreme case: the series being in this hypothesis a little more rapidly convergent than in some others, so that it would be unnecessary to compute more terms if it were to be employed for any practical purpose. It may be remarked that the omission of v^2 in the fourth term is not quite so unimportant to the result as it appeared at first sight, though it is compensated, in the approximation that has been adopted, by the alteration of the other coefficients.

Mr. Ivory, observing with some truth that Dr. Young's inverse series was not in all cases so convergent as could be desired, or even as the author appeared to believe it, has still more lately applied the powerful machinery of his analytical investigations to the construction of some tables upon a hypothesis which seems in most respects to represent the constitution of the atmosphere with sufficient accuracy, and which agrees also extremely well with the most approved observations. Mr. Ivory adopts the opinion of Schmidt, and of some later experimental philosophers, that a diminution of temperature diminishes the actual bulk of a given portion of air by an equal quantity of space for each degree of the thermometer, and infers that, at the temperature of about − 500° of Fahrenheit,

* Supra, No. XXXI.

the air would cease to occupy any space whatever as such, or in the form of a gas. He seems, however, to have imagined in some of his earlier papers, that the diminution of temperature might still be equable in ascending to all possible heights; and even in his essay, printed in the Transactions for 1823, he says, "there is no ground in experience for attributing to the gradation of heat in the atmosphere any other law than that of an equable decrease as the altitude increases . . .: it therefore seems to be the assumption most likely to guide us aright in approximating to the true constitution of the atmosphere." From this hypothesis he derives the very convenient conclusion that the pressure must vary as a certain power of the density, or that $y = z^n$, n being nearly $\frac{5}{4}$: but finding it impossible to suppose the atmosphere so little elevated as this hypothesis would require, he modifies it by the addition of another term to the value of y, though without very clearly relinquishing in words the original supposition, and ultimately adopts an expression equivalent to that which has already been mentioned in this paper, or $y = .75z + .25z^2$.

Mr. Ivory first expands the well known expression for the refraction into a series by means of the binomial theorem, and finds the value of the particular fluents of the several terms from considerations nearly resembling those which Laplace has employed in the Celestial Mechanics, so that the fluent of $e^{-tt}\mathrm{d}t$ becomes a particular case of his solution. Then taking $y = z^{\frac{5}{4}}$, he computes the horizontal refraction 34′ 1″.3 instead of 33′ 51″.5, which is the result commonly adopted; and he finds that at all altitudes the formula differs a few seconds only from the French tables. But this equation supposes the whole height of the atmosphere to be no more than about 25 miles, since $\mathrm{d}y = \frac{5}{4} z^{\frac{1}{4}} \mathrm{d}z$, and $\mathrm{d}x = \frac{-\mathrm{d}y}{mz} = \frac{-5}{4m} z^{-\frac{3}{4}} \mathrm{d}z$, and $\int \mathrm{d}x = \frac{-5}{m} z^{\frac{1}{4}} + \frac{5}{m}$, and $\frac{1}{m}$ is a little more than 5 miles. Mr. Ivory therefore inquires, what would be the effect of an atmosphere in which $y = fz^2 + (1-f)\,z^{1+\frac{1}{n}}$, f being $= \frac{n-4}{4n-4}$, so that when $\frac{1}{n} = 0$, and $n = \infty$, $f = \frac{1}{4}$, and $y = \frac{1}{4} z^2 + \frac{3}{4} z$, the height being thus

made to vary from 25 miles to infinity. But in all these cases he observes, that the rate at which the heat decreases, becomes slower at greater heights than at smaller.

"When n is less than 4, f becomes negative; but these cases are excluded, since they belong to atmospheres still less elevated than when $n=4$. They are excluded too for another reason: for although the rate of the decrease of heat at the earth's surface agrees with nature, yet it increases in ascending, which is contrary to experience." Among these excluded atmospheres is that which supposes $y = \frac{3}{2}z^{3} - \frac{1}{2}z^{2}$:* and no doubt the first objection against its pneumatical accuracy is valid; and Mr. Ivory's expression is more accurate at extreme heights; but there is no ground whatever from experience to deny that the rate of decrement of temperature initially increases: the observations of Humboldt, Leslie, and others, on mountains, have sufficiently shown that the rate increases for the earth's surface, and it will be therefore difficult to show that it must be otherwise for the atmosphere; it is indeed possible that, though the atmosphere is certainly of the same mean temperature as the earth at the base of the mountain, and probably at its summit, it may be a little colder at the middle of its height; but this diversity is by no means shown by any actual observations that have been recorded; and even if Mr. Ivory's hypothesis for the densities be allowed to be most probable, it will not follow that the temperature must not decrease more rapidly at moderate heights than he supposes, in order that the contraction of bulk may keep pace with his formula.

For the atmosphere of infinite height, in which $f = \frac{1}{4}$, Mr. Ivory finds the horizontal refraction 34′ 18″.5, or 17″.2 more than for an atmosphere 25 miles in height, and 27″ more than the quantity generally admitted by astronomers. It seems, therefore, to follow that for an optical hypothesis, an atmosphere *less* than 25 miles high might have the advantage; even if it did not afford the greater facility of direct computation which has since been pointed out.

Having examined the comparative effect of different hypo-

* Supra, No. XXXI.

theses respecting the height of the atmosphere on the refraction, Mr. Ivory proceeds to accommodate his formulas to the more ready computation of the mean refraction, and of the barometrical and thermometrical corrections in the case of the constitution, which appears on the whole to come the nearest to the truth. It seems, however, questionable, whether the value of the exponent of the density $\frac{5}{4}$ is not a little too great, since it is derived from observations on mountains at small heights;* for it is probable that the very summits of the highest mountains that we can ascend ought to be chosen for determining the rate of variation of temperature, if it is to be supposed uniform, and that if we take the exponent $\frac{dz}{dy} = \frac{5}{4}$ only, there will be a deficiency which requires to be compensated, by assuming the rate of variation to become more rapid in ascending; and this seems to be the case in the atmosphere of 18 miles, which agrees so nearly with the results of Laplace's hypothesis, in which $\frac{dz}{dy} = 1.396$.†

There is a mathematical paradox in the latter part of Mr. Ivory's paper, which requires some further explanation.

"The density in the hypothesis of Kramp being too complicated for calculation, he deduces from it," says Mr. Ivory, "this more simple value," $z = e^{-(1-\iota)\sigma}$ "by retaining only the part of the expansion of the function in the index that contains the first power of σ."

In all this Kramp is followed by Bessel, whose aim it is to determine the value of ε that will best represent all the observations of Dr. Bradley, without paying any regard to the terrestrial phenomena, or to any further theoretical considerations whatever.

* Mr. Ivory, in his reply to this historical sketch (Phil. Mag. for 1825, vol. lxv. p. 34), says that this statement is incorrect. "In the average," says he, "I have adopted, all the greatest heights are taken into account, not excepting Gay Lussac's ascent. The result obtained is then compared to the same ascent, which is the greatest height hitherto attained by man; and the difference is shown to be inconsiderable." It has been elsewhere most justly observed by Dr. Young that inasmuch as the elevation which Gay Lussac attained was estimated by the fall of the barometer and thermometer, it would be reasoning in a vicious circle to make it the foundation of any conclusions respecting the relations which connect the height, the density, and the temperature of the air.—*Note by the Editor.*

† Noticed in Brande's Journal, Vol. XV. p. 362, in some remarks on M. Plana's Researches on Refraction.—*Note by the Editor.*

"Now, there is an essential distinction between the rigorous expression of the density, and the approximate value used instead of it. The latter belongs to a finite atmosphere, and the former to one of unlimited extent;... and the total height will be determined by the equation" $e^{-(1-\iota)\sigma} - \varepsilon = 0$.

"At the surface of the earth, we ought to have $\varepsilon = \frac{1}{5}$; which would limit the atmosphere to about double the height in the hypothesis of Cassini. Bessel determines $\varepsilon = \frac{1}{28}$ nearly; which is quite inconsistent with the value of" ζ "at the surface of the earth, and with the elevation necessary for depressing the thermometer one degree, as found by experiment. Accordingly, although the refractions in this table represent Dr. Bradley's observations with great exactness, as far as 86° from the zenith, yet, at lower altitudes, they diverge greatly from the truth."

Now it seems obvious, that since $z = e^{-(1-\iota)\sigma}$, when $e^{-(1-\iota)\sigma} - \varepsilon = 0$, $z - \varepsilon = 0$, and $z = \varepsilon$, instead of $z = 0$. However this may be, Professor Bessel certainly states the result very differently from Mr. Ivory, for he says (*Fund. Astr.*, p. 57).

"Finem huic capiti imponat *stricta* densitatis aeris comparatio, qualis prodit e formula" I, (the correct hypothesis), and II (the approximation).

σ, The height.	ε, The density (I).	ε (II).
625 toises	.8668	.8671
10000	.0921	.1021
20000	.0086	.0104
40000	.0000	.0001.

Now if the atmosphere terminated when $z - \varepsilon = 0$ and $z = \frac{1}{28}$, the heights placed opposite to the last numbers of the table must be merely imaginary. The mistake *appears* to be in the correction of the fluent for y the pressure, which seems without necessity to be supposed initially = 1.*

* Mr. Ivory, in his reply, has satisfactorily explained this paradox: when the density is $e^{-(1-\iota)\sigma}$, the corresponding expressions for the pressure and heat are $\frac{e^{-(1-\iota)\sigma}}{1-\iota} - \frac{\iota}{1-\iota}$ and $\frac{1}{1-\iota} - \frac{\iota e^{(1-\iota)\sigma}}{1-\iota}$ respectively: the properties therefore of the new atmosphere are essentially different from the one originally assumed. —*Note by the Editor.*

Mr. Ivory concludes with approving from theory the employment of the interior thermometer instead of the exterior, a method which Dr. Brinkley thinks himself justified in adopting from observation, but which appears, to some of the best judges in Europe, to be one of the causes that have introduced a variety of mistaken opinions among the most refined discoveries of modern astronomy. The evidence of our senses, when continually repeated, is strong enough to convince us of things the most repugnant to our judgment; but it is not so easy to imagine how, from mere theoretical grounds, it can be believed, that a refraction which has taken place at a horizontal surface in the atmosphere above the observatory can be at all annihilated or compensated by a subsequent refraction at a vertical or greatly inclined surface, which separates the air of different temperatures within and without the observatory: for the laws of equilibrium would never allow the separation to remain in a horizontal direction, or near it, even in the most tempestuous weather.

Mr. Ivory's Table of Refractions would certainly deserve to be annexed to this abstract, if it had not already been examined in this Journal,* and if the correction for temperature, which is so carefully applied, were more supported by observations, compared solely for that purpose. With respect indeed to this correction, it is highly advisable, that every observatory should have it determined from its own observations only: the mean refractions of the French Tables are sufficiently established; though, as Mr. Ivory has discovered, they were actually computed for the freezing temperature, and not very perfectly reduced to the mean temperature to which they are assigned; but they are much better than the labour of many years could procure from the observations of a single astronomer only. It may, however, still be advisable to retain the theoretical correction for temperature in the 'Nautical Almanac,' because a work of that kind, which is likely to be consulted in a variety of climates, is required to represent the probable results of the mean constitution of an atmosphere in equilibrium at different

* See 'Brande's Journal,' vol. xii.: An Apology for the Postscript on Refraction, in answer to Mr. Ivory's remarks. Mr. Ivory asserted that Dr. Young's formula and the table of refractions founded upon it were empirical merely.—*Note by the Editor.*

temperatures depending on climate, and not the temporary effects of the seasons only, or of the weather at the moment, or of the alternations of day and night.

The grounds of Dr. Young's latest method of computing the refraction will be obvious, from comparing this paper with the demonstrations contained in former numbers of this Journal, in which the equations $y = z^2$ and $y = z^{\frac{3}{4}}$ are both shown to afford finite expressions for the refraction; and it appears that their combination, in the form $y = \frac{3}{2}z^{\frac{4}{3}} - \frac{1}{2}z^2$, belongs to an atmosphere which might be expected, from Mr. Ivory's investigation, to represent the refraction with extreme accuracy, though it is probably more dense than the true atmosphere at great heights, and yet terminates too abruptly. But the unexpected advantage of combining a perfect representation of the true decrement of heat at the earth's surface, with a very accurate expression of the refraction, in an equation of a finite form, and not laborious in its application, must at least give this hypothesis some claim to the attention of those who feel any remaining objection to the approximation that has been employed in the 'Nautical Almanac.'

Professor Schumacher is desirous of having it explicitly understood, that the omission of the passage relating to the preference of the exterior thermometer, in his edition of the English explanation of Dr. Young's Table, was completely accidental; it is retained in the German translation, and Professor Schumacher fully coincides in the opinion that it expresses.*

* This refers to the following letter addressed by Dr. Young to Professor Schumacher, which is given in 'Brande's Quarterly Journal,' vol. xvii. p. 103 :—

"I was much surprised the other day to observe that in copying the explanation of my Table of Refractions from the Nautical Almanac, you had omitted, without assigning any reason, the words 'which would be more consistent with the theory,' an expression which I had employed in speaking of the use of the external thermometer, in preference to the interior. I am the more disposed to remonstrate with you on this occasion, because I observe that a great number of astronomers, and among them some who do not usually act without reflection, have inconsiderately taken it for granted that the correction ought to be made according to the height of the interior thermometer, as nearest to the place of observation.

"Now with regard to the theory, it is perfectly obvious, that the computation

extends only to such changes of density as take place between the different strata of the atmosphere considered as *horizontal;* and that its results must necessarily terminate where this regular constitution of the atmosphere ends: that is, *outside* the observatory or other building containing the instruments: while the change of density between the external and internal air, taking place in general at surfaces more nearly vertical than horizontal, at least when the object is but little elevated, and certainly never at horizontal surfaces, will either have no effect at all in increasing the refraction, or as great an effect at higher as at lower altitudes: so that this little irregular addition or diminution can never require to be considered as a part proportional to the whole original mean refraction.

"With regard to practice and observation, I need only refer to M. Delambre's remarks in the 'Connaissance des Tems' for 1819, where he shows that for Mr. Groombridge's observations the mean error of the exterior thermometer is only five-sixths as great as that of the interior."—*Note by the Editor.*

No. XXXIII.

COMPUTATION OF THE

EFFECT OF TERRESTRIAL REFRACTION,

IN THE ACTUAL CONDITION OF THE ATMOSPHERE.

From Brande's Quarterly Journal for 1821, vol. xi. p. 174.

A. It is well known that a projectile, thrown in an oblique direction, will acquire a height equal to the versed sine of twice the angle of elevation, in the circle, of which the diameter is the height due to the velocity: and that its horizontal range will be four times the corresponding sine. Hence it is obvious, that when the direction is nearly horizontal, the radius of curvature of the path of the projectile will be equal to twice the diameter of that circle, or to twice the height due to the velocity, since the chord is twice as great, and the versed sine the same as in the circle, so that the radius must be quadruple.

B. It is also well known, that the tangent of a parabola intersects its diameter at a distance above the vertex, equal to the length of the absciss below it; so that the portion of the absciss below the vertex is half of the part cut off by the tangent.

C. The horizontal ordinate of the parabola, flowing uniformly with the time, is always proportional to the vertical velocity, and the difference of any two proximate ordinates, compared with their length, and the evanescent interval between them, will always give the distance of the intersection of the tangent, according to the common method of finding the tangents of curves; that is, as the difference of the velocities is to the whole velocity, so is the difference of the absciss to the part

cut off by the tangent, or to twice the absciss reckoned from the vertex.

D. Now the velocity of light, considered as a projectile, must be supposed to vary directly as the refractive density; so that we have only to determine what proportion the variation of the refractive density of the atmosphere, in the height of a foot or a yard, bears to the whole refractive density, and to increase the foot or the yard in the same proportion, and we shall obtain the measure of twice the height due to the velocity, or of the radius of the circle of curvature of the ray of light moving horizontally through such an atmosphere.

E. The velocity of light in a vacuum, and in the atmosphere at 50°, with the barometer at 30, varies in the ratio of 3540 to 3541; the height of a homogeneous atmosphere, under these circumstances, is 27,000 feet; and the temperature descends about 1° for every 300 feet that we ascend. Consequently the velocity varies $\frac{1}{3540 \cdot 27000}$ in every foot, as far as the diminution of pressure is concerned, and $\frac{1}{3540 \cdot 300 \cdot 494}$* is to be deducted for every foot, on account of the diminution of temperature, or as much more or less as this diminution is more or less rapid; so that if the change were 1° in $\frac{27000}{494} = 55$ feet, the refraction would be annihilated, and, if still more sudden, there would be a depression or looming, instead of an elevation. But in ordinary circumstances, supposing Professor Leslie's estimate of 1° in 300 to be correct, we have $\frac{1}{3540} \left(\frac{1}{27000} - \frac{1}{148200}\right) = \frac{1}{116873000}$ for the variation in a foot, and consequently, 116873000 feet for the radius of curvature of the ray; which is to the earth's radius, or 20900000 feet, as 5.6 to 1; consequently, the elevation of a distant object must be $\frac{1}{11.2}$ of the angle subtended at the earth's centre, since the angle contained between an arc and its chord is always equal to half the angular extent of the arc.

F. The general temperature of the atmosphere will affect this refraction in so slight a degree, that it may safely be neglected;

* This assumes that air expands or contracts $\frac{1}{494}$th part for every degree of heat: the experiments of Gay Lussac made this expansion $\frac{1}{480}$th part.—*Note by the Editor.*

but it would be always of importance to ascertain, if possible, the comparative temperature at different heights; and whenever it is practicable to find the height h, corresponding to a depression of $1°$, supposing it to be different from 300, we may employ as a divisor, instead of 11.2, the reciprocal of $\frac{1}{3540} \cdot \left(\frac{1}{27000} - \frac{1}{494h}\right)$ divided by 10450000; or the reciprocal of $\frac{10450000}{3540}\left(\frac{1}{27000} - \frac{1}{494h}\right) = \frac{10450000}{3540} \cdot \frac{h - 54.7}{27000h} = \frac{.1093(h - 54.7)}{h} =$ $.1093 - \frac{5.98}{h}$, which, when $h = 300$, becomes $.1093 - .0199 =$ $.0894 = \frac{1}{11.2}$, as before.

No. XXXIV.

REMARKS ON LAPLACE'S LATEST COMPUTATION OF

THE DENSITY OF THE EARTH.

From Brande's Quarterly Journal* for 1820, vol. ix. p. 32.

It cannot but be highly flattering to any native of this country, to have his suggestions on an astronomical subject admitted and adopted by the Marquis de Laplace: but in applying the theory of compressibility to the internal structure of the earth, it appears that this illustrious mathematician has deviated somewhat too widely from the physical conditions of the problem; partly in order to obtain a convenient and elegant formula for expressing the results, and partly, perhaps, because he was not acquainted with all the experiments, by which these conditions are determined.

* In the preceding volume of this Journal there is given an abstract, translated from the 'Connaissance des Tems,' of a Memoir of Laplace on the Figure of the Earth. After referring to investigations and experiments by which it is proved that the Earth is not homogeneous in the interior, and that the density of the strata increases from the surface to the centre, he proceeds as follows:—

"But the earth, though heterogeneous in a mathematical sense, may still be chemically homogeneous, if the increase of density of its strata is caused only by the additional pressure they suffer as they approach towards the centre. It is easy to conceive that the immense weight of the superior strata may considerably increase their density, though they may not be fluid; for it is known that solid bodies are compressed by their own weight. The law of the densities which result from these compressions being unknown, we cannot tell how far the density of the terrestrial strata may be thus increased. The pressure and the heat which we can produce are very small, compared to those which exist at the surface, and in the interior of the sun and stars. It is even impossible for us to have an idea of the effect of these forces, united in those immense bodies. Every thing tends to make us believe that they have existed at one time in a high degree on the earth, and that the phenomena which they have occasioned, modified by their successive diminution, form the present state of the surface of our globe; a state which is nothing more than the element of a curve, of which time is the abscissa, and of which the ordinates will represent the changes that this surface has suffered without ceasing. We are far from knowing the nature of this curve, and we cannot therefore ascend with certainty to the origin of what we observe on the earth; and if, to satisfy the imagination, always troubled by

Instead of proceeding with the calculation upon the analogy of the well known law of the compression of aëriform fluids, which exhibit an elasticity simply proportional to their density, M. Laplace has at once assumed that the elasticity of a solid

ignorance of the cause of the phenomena which interest us, a few conjectures are ventured, it is wise not to offer them except with extreme caution.

"The density of a gas is proportional to its compression, when the temperature remains the same. This law, which is found true within those limits of density, where we have been able to prove it, evidently cannot apply to liquids and solids, of which the density is very great, compared to that of gas, when the pressure is very small, or even nothing. It is natural to suppose that these bodies resist compression the more they are compressed; so that the ratio of the differential of the pressure to that of the density, instead of being constant, as with gases, increases with the density. The most simple function which can represent the ratio, is the first power of the density, multiplied by a constant quantity. It is this which I have adopted, because it unites to the advantage of representing in the simplest manner what we know of the compression of liquids and solids, a facility of calculation in researches on the figure of the earth. Until now, mathematicians have not included in this research the effect resulting from the compression of the strata. Dr. Young has called their attention to this object, by the ingenious remark, that we may thus explain the increase of density of the strata of the terrestrial spheroid. I have supposed that some interest may be excited by the following analysis, from which it appears that it is possible to explain all the known phenomena depending on the law of the density of these strata. These phenomena are the variation of the degrees of the meridian, and of gravity, the precession of the equinoxes, the nutation of the terrestrial axis, the inequalities which the flattening of the earth produces in the motion of the moon, and lastly, the ratio of the mean density of the earth to that of water, which Cavendish has fixed by an admirable experiment at five and a half. In proceeding from the law already announced of the compression of liquids and solids, I find that, if the earth be supposed to be formed of a substance chemically homogeneous, of which the density is $2\frac{1}{2}$ that of common water, and which compressed by a vertical column of its own substance, equal to the millionth part of half the polar axis, will augment in density 5.5345 millionths of its first density, it will account for all the phenomena. The existence of such a body is very admissible, and there are apparently such on the surface of the earth.

"If the earth were entirely formed of water, and if it be supposed, in conformity with the experiments of Canton, that the density of water, at the temperature of ten degrees (50° Fahr.) and compressed by a column of water 10 metres (32.81925 ft.) in height, increases by 44 millionths, the flattening of the earth would be $\frac{1}{180}$; the coefficient of the square of the sine of the latitude in the expression of the length of the seconds pendulum would be 59 ten-thousanths, and the mean density of the earth would be nine times that of water. These results differ from observations by more than the errors to which they are liable.

"I have supposed the temperature uniform throughout the whole extent of the terrestrial spheroid; but it is very possible that the heat is greater towards the centre, and that would be the case if the earth, originally highly heated, were continually cooling. The ignorance in which we are with respect to the internal constitution of this planet, prevents us from calculating the law by which the heat decreases, and the resulting diminution in the mean temperature of climates; but we can prove that this diminution is insensible for the last 2000 years.

"Suppose a space of a constant temperature, containing a sphere having a rotatory motion; and suppose that after a long time the temperature of the space diminishes one degree; the sphere will finally take this new temperature; its mass will not be at all altered, but its dimensions will diminish by a quantity which I will suppose to be a hundred thousandth, a diminution which is nearly that of glass. In consequence of the principle of areas, the sum of the areas, which each molecule of the sphere will describe round its axis of rotation, will be the same in a given time, as before. It is easy to conclude from this, that the angular velocity of rotation will be augmented

body is proportional to the square of the density. Now there seems to be no very good reason why we should suppose the elasticity to increase more rapidly, with the density, in the case of solids or liquids than in that of elastic fluids; and it would be very difficult to demonstrate that it does not even increase less rapidly. As far, however, as any conjecture can be formed from the loose analogy of the elasticity of steam, compared with that of water and ice, the elasticity of a solid might, perhaps, be expected to vary in the sesquiplicate ratio of the density, but certainly not in the duplicate.

by a fifty thousandth. So that, supposing the time of a rotation to be one day, or a hundred thousand decimal seconds, it will be diminished two seconds by the diminution of a degree in the temperature of the space. If we extend this consequence to the earth, and also consider that the duration of the day has not varied, since the time of Hipparchus, by the hundredth of a second, as I have shown by the comparison of observations with the theory of the secular equation of the moon, we shall conclude, that since that time, the variation of the internal heat of the earth is insensible. It is true that the dilatation, the specific heat, the degree of permeability by heat, and the density of the various strata of the earth being unknown, may cause a sensible difference between the results relative to the earth, and those of the sphere we have supposed; according to which the diminution of the hundredth of a second, in the length of the day, would correspond to a diminution of two hundredths of a degree of temperature. But this difference could never extend from two hundredths of a degree, to the tenth; the loss of terrestrial heat corresponding to the diminution of a hundredth of a second in the length of the day. We may observe even that the diminution of the hundredth of a degree, near the surface, supposes a much greater one in the internal strata; for it is known that ultimately the temperature of all the strata diminishes in the same geometric progression, so that the diminution of a degree near the surface corresponds to a much greater diminution in the strata nearer to the centre. The dimensions of the earth, therefore, and its *inertial momentum*, would diminish more than in the case of the sphere we have supposed. Hence it follows, that if, in the course of time, changes are observed in the mean height of the thermometer placed at the bottom of the observatory caves, it must be attributed not to a variation in the mean temperature of the earth, but to change in the climate of Paris, of which the temperature may vary, with many accidental causes. It is remarkable that the discovery of the true cause of the secular equation of the moon, should at the same time make known to us the invariability of the length of the day, and of the mean temperature of the earth, since the time of the most ancient observations.

"This last phenomenon induces us to suppose that the earth has arrived at that permanent temperature which accords with its position in space, and its relation to the sun. It is found by analysis, that whatever the specific heat, the permeability by heat, and the density of the strata of the terrestrial spheroid, the increase of the heat, at a depth very small, compared to the radius of that spheroid, is equal to the product of that depth, by the elevation of the temperature of the surface of the earth, above the state of which I have just spoken, and by a factor independent of the dimensions of the earth, and which depends only on the qualities of its first stratum relative to heat. From what we know of these qualities, we find that if this elevation was many degrees, the increase of heat would be very sensible at depths to which we have penetrated, and where nevertheless it has not been observed."

The memoir of Dr. Young referred to in these observations is 'On the Mean Density of the Earth,' which forms part of the paper (No. XXVIII. of this work) in the Philosophical Transactions for 1819, 'On the Probabilities of Error in Physical Observations,' &c.—*Note by the Editor*.

However this may be, M. Laplace's hypothesis is not correctly applicable to the internal structure of the earth; since it either makes the mean density too small in comparison with that of the surface, or the compressibility at the surface too great; and if this hypothesis actually represented the law of nature, it would follow that the earth is not "chemically homogeneous," but that the specific gravity of the internal parts is naturally greater than that of the external. In this respect the simple analogy of elastic fluids will afford us a result more conformable to observation.

M. Laplace supposes the mean density of the earth to be 5½, according to Mr. Cavendish's experiments, and the superficial density 2¼ only. Now there is absolutely no rock, either primitive or secondary, of which the specific gravity is less than about 2½, and the mean of a great number of rocks gives at least 2¾: so that, allowing for a moderate admixture of metallic substances, we can only consider it as certain that the specific gravity must be between 2½ and 3; and taking 2¾ for Shehallien, the mean density of the earth, according to Maskelyne's observations, and Hutton's computations, ought to be 4.95. The determination of Cavendish, however, is susceptible of greater accuracy; his result is 5.48, and it will be safest to adopt 5.4, as the most probable mean of the two series of experiments.

The superficial compressibility, assumed by M. Laplace, is much greater than can be admitted, according to the experiments of Chladni on sound, and to those which have been made in this country, as belonging to any solid mineral substance whatever. A column of the height of one millionth of the earth's axis is supposed to produce an increase of density amounting to 5.5345 millionths. Now the modulus of elasticity of glass, and of other compact mineral substances, is generally a column of about ten million feet in height; nor has any solid been observed, except ice, in which it stands so low as five million. But ten million feet is nearly half the length of the earth's axis; so that one millionth of the axis would be two millionths of this modulus; and the pressure of such a column would consequently produce a variation of two millionths in the

density of a solid, or at most of 3 or 4 in the most compressible, and in none so much as 5 or $5\frac{1}{2}$. It must therefore be allowed that this part of the hypothesis is inconsistent with direct observation.

There is the less occasion for encountering any of these difficulties, as we shall find that the theory of compressibility, in its original form, is abundantly capable of representing the most probable results of all the observations, which it is intended to connect. The truth of this assertion will appear from the inspection of a table, which shows the compressibility and ellipticity corresponding to different suppositions respecting the specific gravity of the earth's surface, taking 5.4 as sufficiently demonstrated, for the mean density.

Mean density 5.4		*Elasticity as the density.*		
Superficial Density.	Modulus in parts of the semi-axis.	Modulus in thousands of feet.	Central Density.	Ellipticity.
3.13	.5275	11 024	13.35	$\frac{1}{283}$
3.02	.5048	10 550	14.54	$\frac{1}{294}$
2.79	.4699	9 820	15.78	$\frac{1}{303}$
2.60	.4460	9 321	20.10	$\frac{1}{312}$

From this table we may easily deduce the intermediate results by interpolation : thus if the ellipticity were found exactly $\frac{1}{306}$, we should have for the superficial density 2.73, or $2\frac{3}{4}$, and for the height of the modulus 9 650 000 feet.

In these calculations, it has not been necessary to have recourse to any foreign authority or assistance whatever. Dr. Thomson, in his review of the last volume of the *Philosophical Transactions*, has taken the trouble to observe, that Laplace had *before* pursued a similar investigation, although the slightest inspection of the dates of the respective papers might have convinced him that Laplace had done no more than justice, in acknowledging the true source of the theory in question. The geographical elements of the problem have been supplied by

the experiments and observations of Maskelyne and Cavendish, compared with those of General Mudge, Colonel Lambton, and Captain Kater; the computations have been conducted by the assistance of Mr. Ivory's most masterly investigations of the attractions of spheroids, combined with the theory advanced in the *Philosophical Transactions*, together with an auxiliary approximation, for supplying the want of convergence of the series.

It is unnecessary to enter into any inquiry respecting the precession and nutation, as connected with the earth's density, since these effects are known to depend on the ellipticity of the spheroid and of its strata alone, without any regard to the manner in which the density is distributed among them.

London, 2nd Jan., 1820.

No. XXXV.

REMARKS ON THE

ASTRONOMICAL MEASUREMENTS OF THE ANCIENTS.

From Brande's Quarterly Journal for 1822, vol. xiv. p. 190.

THERE is a passage in Plutarch, as quoted by Eusebius in his *Evangelical Preparation*, which determines the distance of the sun from the earth to be about 95 millions of miles, according to Sir William Drummond's computation of the length of the stadium published a few years since in the *Classical Journal.* The circumstance must be allowed to be very remarkable, and seems at first sight to indicate an astonishing precision of observation without the possession of any accurate instruments: but a little consideration is sufficient to convince us, that to an astronomer unprovided with a telescopic micrometer, it was utterly impossible to ascertain an angle of any kind even without a probability of error of half a minute, much more to come within one-tenth of a second of the truth in the measurement of seven or eight seconds. Indeed the very utmost that could be expected from the observation of the moon's disc at the quadratures, would be to make it probable that there was a sensible though a very small parallax, but whether of a second or a minute could certainly not be conjectured without a telescope; and the perfect coincidence of Plutarch's report of the determination of Erastosthenes with the true measure must have been wholly accidental: a conclusion which is still further confirmed by the extreme inaccuracy of the statement of the moon's distance in the same passage, though the moon's parallax was pretty well known to Eratosthenes, as well as the earth's dimensions.

There is on this subject a singular confusion in the remark of Laplace, that Eratosthenes found the difference of latitude of Syene and Alexandria equal to $\frac{1}{50}$ of the circumference: and this distance being estimated at 5000 stadia, Eratosthenes inferred that the whole circumference was 250,000. "But," he continues, "the uncertainty of the value of the stadium, employed by this astronomer, renders it impossible to appreciate the accuracy of this measurement."—*Exp. du Syst. du Monde*, vol. ii.

Now it is highly improbable that Eratosthenes should have committed any *gross* error in the measurement of the length of Egypt from north to south, and we may surely consider it as a *sufficient* determination of the stadium which he employed, that it was $\frac{1}{5000}$ of the difference of latitude between Alexandria and Syene, or between 31° 13′ and 24° 5′, which is 7° 8′, or about 498 English miles; and 50 times this distance is 24,900, giving 7930 miles for the earth's diameter: a result very fairly obtained from the operations of Eratosthenes, and as correct in reality as the distance of the sun in Plutarch has been rendered by accident only. Nor will the variation be material if we take the number 252,000 from Pliny, instead of 250,000 stadia, as the exact extent assigned by Eratosthenes to the earth's circumference.

In referring to some of these numbers, we may observe one thing with respect to the numeration of the Romans, which has not been commonly noticed: that is, that though they had not invented a *decimal* arithmetic, they occasionally employed a *centenary* notation: thus in Pliny's fifth book, chapter ix., we have XXVI.XXXIX mill. passuum, for 2639 miles, and again XV.XLV, for 1545; the word *centena* being omitted, as was also usual in their computations of money; but they do not seem to have made any further step, either upwards or downwards, in the decimal scale.

Dr. Young, in the next number of Brande's Journal, inserted the following note with reference to the observations in the text:—

"Since the publication of the Remarks on the Exposition du Système du Monde, in the last number of this Journal, the illustrious author of that work has favoured the writer of those remarks with a copy of his late republication of the fifth book of the Exposition, under the title of 'Précis de l'Histoire de l'Astronomie.' 8. Paris,

1821.' The passage in question being no longer liable to the objections which were made to it in this Journal, it becomes necessary to insert it here in its improved form.

'The celebrity of his successor Eratosthenes is principally due to his measurement of the earth, which indeed is the first attempt of the kind that has been recorded in the history of astronomy. It is very probable that in much earlier times astronomers had not wholly omitted to make experiments of the same kind; but nothing has been left of these earlier operations, except some estimations of the circumference of the earth, which have been reduced by means of comparisons more ingenious than demonstrative, to something like an agreement with more modern determinations. Eratosthenes having considered that at Syene, the sun, at the time of the summer solstice, shone into a well throughout its depth, and, comparing this observation with that of the meridian altitude of the sun at the same solstice, as observed at Alexandria, found the celestial arc, comprehended between the zeniths of these two cities, equal to the fiftieth part of the whole circumference; and as their distance was estimated at about five thousand stadia, he gave 252,000 stadia as the whole length of the terrestrial meridian. It is, however, very improbable, that, for so important a purpose, this great astronomer should have been contented with the coarse observation of a well enlightened by the sun. This consideration, and the relation of Cleomedes, authorise us to conclude that he observed the shadow of the gnomon at the summer and winter solstices, both at Syene and at Alexandria: and in this manner he obtained the difference of latitude of these two cities very nearly such as it has been found by modern observations. But the greatest uncertainty, that this measurement has left us, relates to the length of the stadium employed by Eratosthenes, which it is difficult to determine among the multitude of different stadia that were employed by the Greeks.'—*Brande's Quarterly Journal*, vol. xiv. pp. 410, 411.

No. XXXVI.

ESTIMATE OF THE EFFECT OF THE TERMS INVOLVING THE SQUARE OF THE DISTURBING FORCE ON THE DETERMINATION OF THE

FIGURE OF THE EARTH.

IN A LETTER TO G. B. AIRY, ESQ.

From Brande's Quarterly Journal for 1826, vol. xxi. p. 346.

MY DEAR SIR,

I VENTURED to express to you the other day my opinion, that the terms depending on the square of the force might safely be neglected in our investigations relating to the figure of the earth; and I shall now state more particularly the reasoning on which my estimate is founded, taking as an example the case which we mentioned, of a fluid supposed to be without weight, and surrounding a spherical nucleus. In this case I apprehend that the consideration of the square of the force will make no difference whatever in the excess of the equatorial diameter above the axis, but that the semidiameter bisecting the angle formed by those lines will be *shortened* by one *half* of the *square* of the ellipticity; that is, for a body of the magnitude of the earth, by about thirty feet.

Calling a minute centrifugal force at the equator f, the force of gravitation there being unity, the immediate centrifugal force elsewhere will be expressed by f cos. φ, φ being the latitude, or, more correctly, the reduced latitude, which has also been called the geocentric latitude, and might be named with more minute propriety *centrocentric;* and the same force, reduced to a horizontal direction, will be f sin. φ cos. φ. Again, the excess of the equatorial semidiameter above the semiaxis 1 being ε, th elevation above the inscribed sphere will be everywhere ε cos.$^2\varphi$: consequently the inclination to the spherical surface or its tangent, will be $\varepsilon \frac{d \cos.^2\varphi}{d\varphi} = -2\varepsilon$ sin. φ cos. φ, which expresses

the force of gravitation reduced to the direction of the surface, and which must be equal to f sin. φ cos. φ, so that we have $2\varepsilon = -f$, when both are evanescent. When they are still small, but not evanescent, we must compute the amount of two perturbations; the first arising from the inclination of the surface, which makes the sine of the angle in the proportion of which the force f is to be reduced, not that of the geocentric, but that of the true latitude; that is, not sin. φ, but sin. φ + cos. φ 2ε sin. φ cos. φ; and in order to counteract this perturbation and to preserve the equilibrium, we must have an additional inclination equivalent to $-f$ cos. $^3\varphi$ 2ε sin. φ, or to f^2 cos. $^3\varphi$ sin. φ. In the second place, the supposed inclination will require to be modified on account of the variation of the force of gravity depending on the distance from the centre, a variation amounting everywhere to 2ε sin. $^2\varphi$, which is the measure of twice the depression below the circumscribed sphere, so that the tangent, instead of f sin. φ cos. φ, must become f sin. φ cos. φ $(1 - f \text{sin.}^2\varphi)$, the alteration being $= f^2$ sin. $^3\varphi$ cos. φ. The sum of the fluents of these two corrections, which are respectively $\frac{1}{4}f^2$ cos.^{4} φ, and $\frac{1}{4}f^2$ sin. $^4\varphi$, shows the elevations, which at the equator and at the poles, are simply $\frac{1}{4}f^2$; and being equal, do not affect the magnitude of the ellipticity. But cos. $^4\varphi$ + sin. $^4\varphi = \frac{3}{4} + \frac{1}{4}$ cos. 4ϕ, the fluxion of which is also expressed by $-$ sin. $4\varphi d\varphi$, and the second fluxion by -4 cos. $4\varphi\, d\varphi^2$, so that a curvature of f^2 cos. 4φ is to be everywhere added to that of the elliptic arc, the curvature of the inscribed sphere being unity; and it is evident that within one fourth of a right angle of the equator and of the poles, this minute quantity will increase the curvature, and diminish it at the intermediate latitudes; the elevation added being always $\frac{3}{16}f^2 + \frac{1}{16}f^2$ cos. 4φ, or $\frac{3}{4}\varepsilon^2 + \frac{1}{4}\varepsilon^2$ cos. 4φ; the utmost variation being $\frac{1}{2}\varepsilon^2$ or $\frac{1}{8}f^2$, and f being $\frac{1}{333}$; so that it can nowhere exceed $\frac{1}{887000}$.

Since, therefore, it is found that in the two extreme cases of a uniform density, and a uniform difference of density between the surface and the central parts, the equilibrium is obtained in a figure not sensibly differing from an elliptic spheroid, it may safely be concluded that the ellipsis will sufficiently answer the conditions of equilibrium in intermediate constitutions of the internal parts of the earth.

No. XXXVII.

SIMPLE DETERMINATION OF THE MOST ANCIENT EPOCH OF

ASTRONOMICAL CHRONOLOGY.

In a Letter to Francis Baily, Esq., F.R.S.

From Brande's Quarterly Journal, vol. xxv. p. 195.

My dear Sir,

When I addressed to you some remarks on the date of an astrological manuscript found in Egypt, I was not aware how perfectly superfluous the chronological evidence, afforded by such fragments, is rendered by the accuracy of the original tables of Ptolemy, which were probably the basis of the computations that those fragments contain. I have since looked into these tables, as they are exhibited in the edition of Basil, which, without any suspicion of having been sophisticated by translators or commentators, is still very correctly printed; and my copy of which I read with the greater pleasure, as a gift of my friend, Professor Schumacher: you have also had the goodness to furnish me with the elaborate Commentaries of the Abbé Halma, which I did not venture to consult until I had made a separate computation of the chief points that I wished to ascertain: although I afterwards obtained from them some valuable assistance in verifying my results, and in the more ready comparison of the different parts of the wonderful original with each other.

The planetary tables of Ptolemy are all carried back to the epoch of the Alexandrian noon of the first day of the first Egyptian year of the reign of Nabonassar. The mean daily motion of Saturn, as laid down in these tables, is 0° 2′ 0″ 33‴ 31⁗ 28′′′′′ 51′′′′′′; and that of Jupiter 0° 4′ 59″ 14‴ 26⁗ 46′′′′′ 31′′′′′′; (P. 214, 215, 217, 218.) The former of these motions is less by $\frac{1}{3700}$, the latter by $\frac{1}{12000}$ only, than those which are laid down by Bouvard in the latest of our modern tables. They therefore afford very convenient foundations for determining the exact year that was intended; and

their evidence is of the more importance, as the other very slow changes, which might be employed for the same purpose, indicate, for some unknown reason, a much greater antiquity than is consistent with collateral evidence; the changes in the position of the earth's axis, for example, both in its direction and its inclination, being considerably greater than the results of the best theories would lead us to expect in the time that has elapsed.

We may suppose, in the first instance, that the epoch of the sun's mean longitude (P. 81) is correctly laid down as 330° 45′, and his true longitude 333° 8′; the great equation, 2° 23′, being found in the table of Prosthaphaereses (P. 78), opposite to the mean anomaly 265° 15′: then, taking the sun's mean longitude at the beginning of the corresponding Gregorian year, as about 280°, the difference in the sun's longitude becomes about 50° 45′; and it will be most convenient to reduce the places of the planet to the beginning of such a year, in order to compare them with the modern tables, which are arranged according to Julian years; the difference of these years not being material for the present purpose. We thus obtain the epochs of the tables, which are 296° 43′ and 184° 41′ (P. 213, 216), 295° for Saturn, and 180° 23′ for Jupiter; that is, in the decimal notation of Bouvard, about 328^{GR} and 200^{GR} respectively.

We now find that Saturn had returned nearly to the same mean longitude at the beginning of 1814: for which we have 327.07^{GR}: and if we look back for all the years at the beginning of which the longitude is the same within a very few degrees, we shall observe recurrences more or less exact at periods of 2, of 7, and of 12 revolutions, corresponding to 59, 206, and 353 years; and we may easily make a table of all those which particularly require attention.

Year.	♄ GR.	Year.	♄ GR.	Year.	♄ GR.
1814	327	−657	335	− 863	335
1461		−658	322	− 864	322
1108		−687	327	− 893	327
755		−716	333	− 922	333
402		−746	325.6	− 952	326
49		−775	331	− 981	331
−304		−805	323	−1011	323
		−834	329	−1364	

Among these dates, those which afford the nearest coincidences are −893, −834, −687, and −746: and it is sufficiently well known that this last is the true date, as we may at once infer from the place of Jupiter, which is 198.5^{GR} at the beginning of this year, and in −805, the nearest alternative, 208^{GR}; in −687, 189^{GR} only.

If we wish to verify the calculation, from Ptolemy's own tables of Saturn, we have, for the 2560 equinoctial years between −746 and 1814, 2560×365.24222 days, making 2561 Egyptian years, and 255.1 days. Then (P. 213)

For $2268^y = 324 \times 7$	2°	29′	0″
288	280	18	55
5 (P. 214)	61	7	0
240^d (P. 215)	8	2	14
15.1	0	30	50
	352	7	29

This is too little by $7\frac{1}{2}$°, or $\frac{1}{48}$ of a revolution, out of 87 entire ones, and the agreement would be more perfect if we supposed the time a year longer: but the motion being already slower than that of the modern tables, it is clear that such a supposition is inadmissible. In a similar manner we find, for Jupiter, the longitude in 1827, 202^{GR}, after 2574 Egyptian years and $258\frac{1}{4}$ days

Hence, (P. 216, 218) $2430^y = 810 \times 3$	285°	26′	42″
144	48	54	55
240^d	19	56	58
$18\frac{1}{4}$	1	31	1
	355	49	36

The error here appears to be only 4° in 257 revolutions, which is little more than $\frac{1}{20000}$; but, in fact, it is a degree or two greater at each end, though still small enough to make the year perfectly certain.

It is therefore abundantly demonstrated, from the tables of Saturn and Jupiter only, that the Alexandrian noon of the first day of the first year of Nabonassar happened in the equinoctial

year preceding the vernal equinox -746; and, according to Ptolemy's reckoning, the sun's mean longitude was 330° 45′, whence the date was M. Eq. -746^{y} -29.676^{d}: or, since, according to Ptolemy's equation, the true equinox happened when the mean longitude was 359° 23′ only, if we call the true equinox $\underset{\Upsilon}{\odot}$, the date becomes $\underset{\Upsilon}{\odot} - 746^{y} - 29.050^{d}$: and this date is less likely to be affected by the error of the equation than the former; but they both require correction on account of the erroneous length of the year employed in carrying back the epoch: for though Ptolemy's sidereal year is within 12 seconds of the truth, his tropical year, as well as that of Hipparchus, is about five minutes too long: since these great astronomers agree in making the Julian year too long by $\frac{1}{300}$ of a day, while the Gregorian is shorter by $\frac{3}{400}$; so that in the 600 years preceding the most accurate observations of Hipparchus, they made this difference 2 days instead of 4½, and they supposed the sun to have been 2½ days too far advanced on the day in question, and to have described a space so much shorter than the truth. This correction would give us the date $\underset{\Upsilon}{\odot} - 746^{y} - 31.55^{d}$. But the mean of Hipparchus's actual observations, reduced, with all possible care, according to the correct value of the tropical year, gives us nearly $\underset{\Upsilon}{\odot} - 746^{y} - 30.4^{d}$ for the epoch of Nabonassar.

With the assistance of Mr. Halma's table of astronomical chronology, and of the Memoirs of Professor Ideler, which he has republished, I have endeavoured to exhibit, in chronological order, the various observations which are scattered through the works of Ptolemy, and to connect them with the series of Olympiads, and with other chronological epochs. But I must defer this table to a future occasion.*

Believe me, dear Sir,

Yours very sincerely,

T. Y.

Park Square, 8 March, 1828.

* It appeared in the next number of Brande's Journal under the title 'Astronomical Chronology, deduced from Ptolemy and his Commentators.'—*Note by the Editor.*

No. XXXVIII.

ON THE RESISTANCE OF THE AIR.

DETERMINED FROM CAPTAIN KATER'S EXPERIMENTS ON THE PENDULUM.

From Brande's Quarterly Journal for 1823, vol. xv. p. 351.

The effect of resistances of various kinds on the vibrations of the pendulum is become a subject of increased importance, from its influence on the determination of a standard measure: for although the effect of these resistances on the time may be wholly inconsiderable, it is by no means superfluous to prove, by demonstrative evidence, that they are actually insensible.

A constant resistance, and a resistance proportional to the square of the velocity, produce either no change at all of the time of vibration, or an infinitely small change when the arc is infinitely small: but a resistance simply proportional to the velocity, if it be at all considerable, may produce a sensible retardation, even in an evanescent arc. It becomes, therefore, of some importance to inquire, what is the law of the resistance to very slow motions; and the elaborate experiments of the indefatigable Captain Kater will afford us the information that is required for establishing, in this respect, the sufficiency of the superstructure that has been built on them. It is, however, necessary to take the mean of a large number of separate registers of observations, in order to investigate the laws of the retardation: for the question is so delicate, that the results of any small number of experiments might lead to very erroneous conclusions: but when properly analysed, the experiments, related in the third part of the *Philosophical Transactions* for 1819, are amply sufficient to show that a certain portion of the resistance to the motion varies simply as the velo-

city; and that it cannot be correctly expressed, as Mr. Gilbert has supposed,* by a constant term and a term proportional to the square of the velocity only. Sir Isaac Newton indeed, has hinted in the *Principia*, that a constant term expressing the resistance derived from the thread suspending his pendulum, with another term proportional to the square of the velocity, might be sufficiently accurate for the purpose; and Euler has inferred, from Newton's experiments, that the constant resistance of the air to the motion of a leaden ball, two inches in diameter, was about one millionth part of its weight, or that it would cause it to remain at rest at an angular deviation of $0''.2$ from the vertical line: but a part at least of this resistance may perhaps have been derived from the want of flexibility or elasticity of the thread.

From a mean of 60 experiments of Captain Kater, consisting of about 5000 vibrations each, we obtain $1^\circ.185$, $1^\circ.086$, $0^\circ.997$, $0^\circ.919$, and $0^\circ.843$ for the successive values of the arcs, at intervals of about 960 vibrations: and a slight irregularity in the second differences of these numbers makes it probable that .997 ought to be altered to .998. With this correction, the successive diminutions, in about 1920 vibrations, will be .187, .167, and .154, for the respective arcs of intermediate values, each of which must be supposed to exceed the intermediate arc actually observed by one third of its deficiency below the mean of the two neighbouring numbers, and we may call them 1.088, 1.000, and .9195 respectively.

Putting then $D = x + Ay + A^2z$, for the diminution of the arc, we have three equations, the last of which, subtracted from the first, gives us $.1685\ (y + 2.1075\ z) = .033$, and $y + 2\cdot1075\ z = .1958$; consequently, if $z = 0$, $y = .196$, which would be the coefficient for a resistance simply proportional to the arc, giving $x + .196$ for the amount of the second diminution, that is, .167; so that x would require to be negative, which is impossible: and if $y = 0$, $z = .093$, and the second

* Referring to an article in the same volume of this Journal, p. 90, 'On the Vibrations of Heavy Bodies in Cycloidal and Circular Arches, as compared with their descents through free space: including an estimate of the Variable Circular Excess in Vibrations continually decreasing.' By Davies Gilbert. . . . With a Supplement. —*Note by the Editor.*

diminution would require x to be .074: a value which is sufficiently compatible with these equations, but which would not be applicable to the shorter vibrations; an arc of 0.°80, for example, exhibiting a diminution of about .11, and leaving only about .050 for x, so that x must probably be still smaller than .05, and if we make it = .040, we shall have 127 left for $y + z$ and $.196 - .127 = .069 = 1.1075\ z$, and $z = .062$, and $y = .065$, and $D = .040 + .065\ A + .062\ A^2$, which gives 132 for an arc of .8, and x is still too large. Now, if we take x somewhat smaller, we shall reduce the expression to a perfect square, and we shall find that $(.16 + .25\ A)^2 = .0256 + .080\ A + .0625\ A^2$ will represent the diminution with great accuracy, giving .187, .168, and .152, for the respective arcs of 1.09, 1.00, and .92: and this expression has the advantage of affording a very easy integration for the arc

For, if t be the number of vibrations divided by 1920, we have $-\ dA = (.16 + .25A)^2\ dt$, and $\frac{-dA}{(.16+.25A)^2} = dt$: but $d\,\frac{1}{.16+.25A} = \frac{-.25dA}{(.16+.25A)^2}$, and therefore $\frac{4}{.16+.25A} = t + c$ or $.16 + .25\ A = \frac{4}{t+c}$, and $.64 + A = \frac{16}{t+c}$: whence, putting $64 + A = B$ and its initial value b, $b = \frac{16}{c}$, and $c = \frac{16}{b}$; consequently $B = \frac{16}{\frac{16}{b}+t}$, and $\frac{1}{B} = \frac{1}{b} + \frac{t}{16}$.

In many of the series of experiments, it is necessary to make some variation in the constant coefficients, on account of the state of the atmosphere, and we may take in general $B = A + C$, and $\frac{1}{B} = \frac{1}{b} + \frac{t}{q}$, the factor q, in the case already computed, being made either 16, or 16×1920, accordingly as we wish to take the interval of the coincidences for the unit of time, or to express it in seconds; and C, in some of the series of experiments, appearing to be about 1° or even 2°, instead of 0°.64. The supposition of $C = 1°$ is equivalent to that of $D = .04 + .04\ A + .01\ A^2$, q becoming in this case 25.4 instead of 16. The constant part of D, expressed by x, causes, in half a vibration, a retardation of $\frac{1}{3840}x = 0°.000067 = 0'.004 = 0''.24$,

which happens to agree singularly well with the 0.″20 deduced by Euler from Newton's experiments.

We may easily compute, from the value of A thus determined, the total retardation depending on the vibration in a circular curve, which is expressed, for a small arc of vibration, by one-eighth of the versed sine, the whole time of the vibration being unity, or, for the arc A, since the versed sine of 1° is .000152, by very nearly .000019 A^2; and the fluxion of the time being dt, that of the circular excess will be as $A^2dt = (B - C)^2dt = C^2dt - 2BCdt + B^2dt$: now $\frac{b}{B} = 1 + \frac{b}{q}t$ or $= 1 + pt$, putting $p = \frac{b}{q}$, and $B = b\frac{1}{1+pt}$, and $\int \frac{dt}{1+pt} = \frac{1}{p}$ hl $(1+pt)$, consequently the fluent of the second term is $-2C\frac{b}{p}$ hl $(1+pt) = -2Cq$ hl $\frac{b}{B}$; that of the third, or $\frac{bb}{(1+pt)^2}dt$, being, when corrected, $\frac{bb}{p}$. $\frac{pt}{1+pt} = b^2\frac{t}{1+pt} = b^2t\frac{B}{b} = bBt$; so that the whole circular excess will become $.000019t\left(C^2 - 2C\frac{b}{pt}\text{ hl }(1+pt) + \frac{bb}{1+pt}\right)$ or $.000019t\left(-.41 - 1.28\frac{q}{t}\text{ hl }\frac{b}{B} + bB\right) = .00001\left(1.9bB - 2.432\frac{q}{t}\text{ hl }\frac{b}{B} + .779.\right)$ Taking, for example, Captain Kater's first register of experiments, in which $a = 1°.38$, and A .92, when t was $\frac{5}{2}$, so that $\frac{b}{B}$ being $\frac{2.02}{1.56} = 1.2949 = 1 + \frac{b}{q}t = 1 + \frac{5.05}{q}$; we must here make $q = \frac{5.05}{.2949} = 17.124$, and $\frac{q}{t}$ 6.850, and hl $\frac{b}{B}$ being $= .7031 - .4447 = .2584$, the whole is $.00001(5.987 - 4.304 + .779)\,t = .00002462$, or 2.12 in 86050 vibrations; which agrees exactly with Captain Kater's computation from the separate arcs observed.

If we adopted the Newtonian hypothesis of a resistance measured by $m + nA^2$, we should have $\frac{-dA}{m+nA^2} = dt$, and $t = -\sqrt{\frac{1}{mn}}$ arc tang. $\left(\sqrt{\frac{n}{m}}A\right)$, consequently $\sqrt{(mn)}\,t = -$ arc tang. $\left(\sqrt{\frac{n}{m}}A\right)$, and tang. $\sqrt{(mn)}t, = -\sqrt{\frac{n}{m}}A$ and $A = -\sqrt{\frac{m}{n}}$

tang. $(\sqrt{(mn)}\, t)+c$; and for the correction of the fluent, $a = +c$, and $A = a - \sqrt{\frac{m}{n}}$ tang. $(\sqrt{(mn)}t)$.

There appears to be an oversight in a remark inserted among the *Elementary Illustrations of the Celestial Mechanics*, p. 145; where it is observed that "the whole time of the oscillation can never be sensibly affected by any small resistance proportional to the velocity;" for, in fact, the coefficient γ, in the expression of Laplace, being equal to $\sqrt{\left(k - \frac{mm}{4}\right)}$, is in some degree affected by m, which expresses the resistance; and the time is affected by γ, though Laplace has not investigated the precise effects of a given resistance. That which is here inferred from Captain Kater's experiments, however, would scarcely produce a retardation of one fiftieth of a second in a year; and may, therefore, be wholly neglected.

If we are anxious to reconcile the existence of a retardation proportional to the velocity, with the common theory of the impulse of fluids, it will not be difficult to understand how the one may possibly be derived from the other. We have only to suppose the pendulum subjected to the influence of a very slow current of air, in order to deduce a resistance nearly proportional to the velocity v from another, which depends on $(c \mp v)^2$. For it will appear, by considering the directions of the forces concerned, that the extremities of the vibration, while the velocity of the current exceeds that of the pendulum, and $c - v$ remains positive, the quantity $2cv$ will denote a retarding force throughout the motion, and that the portions c^2 and v^2 will be retarding in one direction and accelerating in the other, and will have no sensible effect on the extent of the vibrations; while on the other hand, if the velocity of the pendulum towards the middle of the vibration exceeds that of the current, the force $2cv$ will retard the motion in one direction, and accelerate it in the other, leaving only the constant resistance c^2, and the variable quantity v^2, which is proportional to the square of the velocity. We obtain, therefore, for the extremities of the vibrations, a force proportional to the simple velocity, and for the middle, a constant resistance, and another force varying

simply as the velocity, the joint effect of all which must be a resistance nearly such as has been inferred from Captain Kater's experiments, if the current moved at the rate of about half an inch in a second, which would have been scarcely perceptible to the senses.

The question, however, regards not so much the distribution of the resistance through the different parts of a single vibration, as its comparative value for the mean velocities of the successive vibrations. Now, if the velocity of the current always exceeds that of the pendulum, the only effective resistance will be proportional to the simple velocity; and when it is smaller than the greatest velocity of the pendulum, the resistance will approach more and more to the ratio of the square of the velocities increased by a constant quantity; and supposing the velocity of the current to remain small and nearly uniform, while the arc of vibration considerably diminishes, the whole resistance will at first be more nearly as the square of the arc, and if the arc be sufficiently diminished, the resistance proportional to the simple velocity will at last remain alone. Hence, it is easy to understand the variation of the constant coefficients in the different series of Captain Kater's experiments.*

12 April, 1823.

* This very difficult and important subject, involving some corrections which were not appreciated or foreseen at the time the article in the text was written, has been much more thoroughly investigated in later times by Colonel Sabine, Bessel, Poisson, and more recently by Professor Stokes in an admirable Memoir in the 'Cambridge Transactions for 1850.'—*Note by the Editor.*

No. XXXIX.

CONSIDERATIONS ON THE REDUCTION OF

THE LENGTH OF THE PENDULUM

TO THE LEVEL OF THE SEA.

From Brande's Quarterly Journal for 1826, vol. xxi. p. 167.

Mr. Laplace seems to entertain some doubts of the propriety of considering the density of an elevated portion of the earth's surface, in reducing the length of the pendulum, observed on it, to the level of the sea. The respect due to the opinions of so illustrious a mathematician, renders it therefore necessary to enter into some further explanations on this subject.

If the earth be considered as a sphere, either of uniform density, or disposed in concentric strata, except at a small part near the end of one of its radii, where we may suppose a spherule to be situated of a density so much greater as to exceed that of the neighbouring parts by the mean density, and touching the surface internally; the distance of its centre from that of the earth being a; then at the angular distance ϕ from the given radius, and at the distance z from the centre of the spherule, the direct attraction will be $\frac{n^3}{zz}$, n being $= 1 - a$, or the radius of the spherule, and the angular deflection of the pendulum, or of the surface of the sea, or of that of an atmosphere, the disturbing force $\frac{n}{zz}$ being reduced in the ratio of the sine of the angle subtended by the side a, will be $\frac{n^3}{zz} \cdot \frac{a \sin. \phi}{z} = \frac{an^3 \sin. \phi}{z^3}$; and the fluxion of the elevation correspond-

ing to $d\varphi$, the angle being very small, will be $\frac{an^3 \sin. \varphi}{z^3} d\varphi$. Now $z^2 = (\cos. \varphi - a)^2 + \sin.^2\varphi = \cos.^2\varphi + a^2 - 2a \cos. \varphi + \sin.^2\varphi = 1 + a^2 - 2a \cos. \varphi$; and the fluxion of $\frac{1}{\sqrt{(1 + a^2 - 2a \cos. \varphi)}}$ is $-\frac{a \sin. \varphi d\varphi}{z^3}$; consequently the fluent of $\frac{an^3 \sin. \varphi}{z^3} d\varphi$ is $\frac{n^3}{z}$. Making, for example, $a = .999$, as for a spherule about 8 miles in diameter, and $n = .001$, the fluent at the remotest extremity is $\frac{.000000001}{1.999}$; and taking the distance $z = 1$ as giving about the mean level of the sea, the correction will be $\left(\frac{.001}{1}\right)^3$ and the elevation, over the spherule, $(.001)^2 - (.001)^3$, or nearly one millionth of the radius; that is about 21 feet. Such would therefore be the elevation of the true level of the sea by the attraction of the supposed spherule; and whether we reduced the length of a pendulum to this level, or to the surface of the sphere, the difference would be insensible; while the real force of gravity, and consequently the length of the pendulum, would be increased one thousandth by the presence of such a spherule, and the curvature of the meridian at the spot would be deranged in a still greater degree.

If the sphere were now contracted to the radius a, and the surface were covered with an atmosphere of the height of the diameter of the spherule, it is obvious that the spherule remaining in its place would become a mountain of the mean density of the earth, and the surface of the atmosphere would still be in equilibrium at the height of 21 feet only above the original surface of the sphere: and the spherule might be flattened into a table-land, without any sensible alteration of its action, provided that the place of its centre of gravity remained but little changed. In any case the length of the pendulum at the surface of the globular mou .tain, or of the table-land, would be manifestly affected by the attraction of the prominence, and consequently by its density, while the actual elevation or depression of the level of the sea would comparatively be very inconsiderable: this elevation may therefore safely be neglected in practical cases: while it is impossible to compare the length of the pendulum in different latitudes, as

referred to any regular spheroid, in a satisfactory manner, without first making corrections for the effect of such accidental irregularities, as far as it is in our power to ascertain them.

It must however be observed, that the correction, taken in this general sense, cannot be considered as coming under the denomination of a reduction to the *local* level of the sea. In the case of the sphere, for example, diminished to $1-2n$, the length of the pendulum at the general level of the sea would be $1+4n$, at the summit of the globular mountain $1+n$ only; immediately below it, $1+n-4n=1-3n$, while if we allowed for the height only, without considering the attraction of the mountain, it would be supposed $1+n+4n=1+5n$, as much exceeding the required value, belonging to the general level of the sea, and independent of the local attraction, as the result of the actual experiment under the mountain would fall short of it.

9, Park Square, Portland Place,
16 March, 1826.

No. XL.

COMPUTATIONS FOR

CLEARING THE COMPASS

OF THE REGULAR EFFECT OF A SHIP'S PERMANENT ATTRACTION.

From Brande's Quarterly Journal for 1820, vol. ix. p. 372.

Investigation.

1. A sufficient approximation, for the explanation of many of the phenomena of the dipping needle, is obtained by supposing the magnetism of the earth to be concentrated into two magnetic poles, very near to each other, and to the earth's centre; this supposition being also equivalent to that of an infinite number of small magnets, parallel to each other, distributed equally throughout the earth's surface, or through any other concentric strata.

2. The angular distance of any point on the earth's surface, from the equator belonging to these poles, being called the magnetic latitude, it has been demonstrated by several mathematicians that the tangent of the dip must be twice the tangent of the magnetic latitude.

3. Hence it may be inferred, that if the sine of the dip be called s, that of the magnetic latitude will be $\frac{s}{\sqrt{(4-3ss)}}$.

4. The angle subtended at any point by the two poles will obviously vary as the cosine of the magnetic latitude.

5. Consequently, in the triangle representing the two magnetic forces and their result, either of the two greater angles being ultimately equal to the complement of the dip, it follows that as the cosine of the dip is to the earth's radius, so is the sine of the small angle subtended by the two poles, to the side

corresponding to the ultimate magnetic force in the direction of the dipping needle.

6. The magnetic force in the direction of the dipping needle will therefore vary as the cosine of the magnetic latitude directly, and inversely as the cosine of the dip, or as $\frac{\cos. L}{\cos. D}$; or since $\cos. L = \frac{\sin. L}{\tan. L}$, as $\frac{\sin. L}{\tan. L} \cdot \frac{\tan. D}{\sin. D} = \frac{\tan. D}{\tan. L} \cdot \frac{\sin. L}{\sin. D} = 2\frac{\sin. L}{\sin. D} = \frac{2}{\sqrt{(4-3ss)}}$; and the magnetic force must vary inversely, as the square root of 4 diminished by three times the square of the sine of the dip: so that between the magnetic equator and the magnetic pole, the force ought to vary in the proportion of 1 to 2, and the vibrations of a given needle, in a given time, ought to vary in that of 10 to 14.142.

7. This variation of the force is greater than has yet been observed: but on board of the *Isabella*, when the dip increased from 74° 23′ to about 86°, the time of vibration decreased in the proportion of 470 to 436, or 1.078 to 1, and consequently the force increased in that of 1.162 to 1, while the calculation requires an increase in the ratio of 1.095 to 1 only; so that, considering the unavoidable uncertainties of the experiment, the general result of observations, in different parts of the globe, agrees as well with the theory as we have any right to expect, and justifies us in introducing this variation of the force into our calculations, at least as an approximate expression of the facts, *to be compared hereafter with more extensive experience.*

8. The force acting on the needle of a compass, limited to a horizontal motion, is reduced, according to the principles of the resolution of forces, in the ratio of the radius to the cosine of the dip, so that it becomes proportional to $\sqrt{\frac{1-ss}{4-3ss}}$, or inversely to $\sqrt{\frac{4-3ss}{1-ss}} = \sqrt{\left(\frac{1}{1-ss} + 3\right)}$.

9. Such being then the magnitude of the horizontal force acting in the direction of the magnetic meridian, we may readily determine the effect of its combination with another force acting in any other direction, so as to afford a result expressed by the third side of the triangle of forces: for the sine of the angle, formed by this new result with the first line, will be to the sine of the angle which it forms with the second, as the second line to the first; or, in other words, the sine of the angle formed by

the actual direction of the needle, with that which would have been its direction if the magnetic force had been undisturbed, will be to the sine of the angle included by its actual direction, and the direction of the disturbing force, as the magnitude of the disturbing force to that of the natural force; and supposing the disturbing force of the ship to be constant in different parts of the globe, the sine of the angular correction, required for its effect, will vary directly as the sine of the angular distance of the needle from the ship's head, or from any other given neutral line in which the disturbing force of the ship is found by experiment to act, and inversely as the magnitude of the horizontal magnetic force; that is directly as $r \surd \left(\frac{1}{1-ss} + 3\right)$; r being the sine of the bearing of the ship's head, or other "point of change," as ascertained by the actual indication of the compass, and not by the corrected bearing, which has sometimes been employed in a similar calculation, and s the sine of the dip; the quantity under the radical sine being equal to the square of the secant of the dip increased by 3.

10. If, for example, the utmost disturbance were found to be 5° 40′, where the dip is 74° 23′, its sine would require to be increased, when the dip became 86°, in the ratio of 1 to 3.523, and the maximum of disturbance would become 20° 21′. It is scarcely possible that the calculation should agree better with the result of the observations made on board of the *Isabella*: so that we may employ it, with *some* confidence, for our assistance, in correcting the errors arising from the disturbing force of the ship in all ordinary cases.

11. When the ship's attraction is constant, it is obvious that the two neutral positions, in which it produces no disturbance, will be observed when the ship's head is exactly in opposite directions. But it appears that there is sometimes also an irregular attraction, causing the two neutral points to be within 8 or 10 points of each other; and when this happens, we can only rely on immediate observation in different parts of the globe for determining the requisite corrections. This part of the disturbance, however, seems not to increase with the dip, and there is every reason to attribute it to the temporary or induced magnetism of some portions of soft iron; since it may

easily be shown, for example, that a horizontal bar of soft iron will lose its effect on the needle in four positions, at right angles to each other, and a bar so inclined, as to become perpendicular to the dipping needle in the plane of the meridian, will lose its effect in its two opposite positions, in that plane, only, but will act with very different intensities in their neighbourhoods, so as to produce different effects in positions diametrically opposite to each other; and from various combinations of such pieces, differently situated, we may easily imagine that all the irregularities, observed in some very few cases, may have originated.

12. Table of Corrections for clearing the Compass of the regular effect of a Ship's Permanent Attraction.

Dip.	Apparent distance of the Ship's head or other Neutral Point from the magnetic North, in points							
	1 15	2 14	3 13	4 12	5 11	6 10	7 9	8 8
0°	.5913	.8839	1.0458	1.1505	1.2209	1.2666	1.2926	1.3010
10	.5930	.8856	1.0475	1.1522	1.2226	1.2683	1.2943	1.3027
20	.5983	.8909	1.0528	1.1576	1.2280	1 2737	1.2997	1.3081
30	.6087	.9013	1.0632	1.1679	1.2383	1.2840	1.3100	1.3184
40	.6265	.9191	1.0810	1.1857	1.2561	1.3019	1.3278	1.3362
50	.6573	.9499	1.1118	1.2165	1.2869	1.3326	1.3586	1.3670
60	.7128	1.0054	1.1673	1.2721	1.3424	1.3882	1.4141	1.4226
65	.7575	1.0501	1.2120	1.3167	1.3871	1.4328	1.4588	1.4672
70	.8215	1.1141	1.2760	1.3808	1.4511	1.4969	1.5228	1.5313
71	.8375	1.1301	1.2920	1.3968	1.4672	1.5129	1.5389	1.5473
72	.8550	1.1476	1.3095	1.4142	1.4846	1.5303	1.5563	1.5647
73	.8739	1.1665	1.3284	1.4331	1.5035	1.5492	1.5752	1.5836
74	.8945	1.1871	1.3490	1.4538	1.5241	1.5699	1.5958	1.6043
75	.9170	1.2096	1.3715	1.4763	1.5466	1.5924	1.6183	1.6268
76	.9417	1.2343	1.3962	1.5010	1.5713	1.6171	1.6430	1.6515
77	.9688	1.2614	1.4233	1.5281	1.5985	1.6442	1.6702	1.6786
78	.9988	1.2914	1.4533	1.5581	1.6285	1.6742	1.7002	1.7086
79	1.0322	1.3248	1.4867	1.5914	1.6618	1.7075	1.7335	1.7419
80	1.0694	1.3620	1.5239	1.6286	1.6990	1.7447	1.7707	1.7791
81	1.1113	1.4039	1.5658	1.6706	1.7409	1.7867	1.8126	1.8211
82	1.1590	1.4516	1.6135	1.7182	1.7886	1.8343	1.8603	1.8687
83	1.2138	1.5064	1.6683	1.7731	1.8434	1.8892	1.9151	1.9236
84	1.2780	1.5706	1.7325	1.8373	1.9076	1.9534	1.9793	1.9878
85	1.3539	1.6465	1.8084	1.9131	1.9835	2.0293	2.0552	2.0636
86	1.4498	1.7424	1.9043	2.0091	2.0794	2.1252	2.1511	2.1596
87	1.5732	1.8658	2.0277	2.1325	2.2028	2.2486	2.2746	2.2830
88	1.7482	2.0408	2.2027	2.3075	2.3778	2.4236	2.4495	2.4580
89	2.0486	2.3412	2.5031	2.6078	2.6782	2.7240	2.7499	2.7583

Use of the Table.

Find by observation the greatest disturbance produced by the ship's action on the compass in any given part of the globe, and subtract from the logarithm of its sine the number in the last column of the table opposite to the given dip, the difference will be the logarithm of the constant multiplier for that ship; and if it be added to the tabular number for any other place, or for any other position of the ship, it will give the logarithmic sine of the correction required on account of the *permanent* attraction.

Example.

Supposing the utmost disturbance in the *Isabella* to be 5° 40′, when the dip is 74° 23′; the numbers of the last column for 74° and 75° being 1.6043 and 1.6268, the difference .0223 becomes, for 23′ .0085, and for 74° 23′ we have 1.6128, which, subtracted from 8.9945, the logarithmic sign of 5° 40′, leaves 7.3817, the logarithm of the constant multiplier for the *Isabella*; then the greatest tabular number for the dip 86° being 2.1596, adding this to 7.3817, the sum 9.5413 is the logarithmic sine of 20° 21′, the greatest disturbance where the dip is 86°; and when the ship's head, or the neutral point, or point of change, appears by the compass to be N.E., or 4 points from the magnetic North, the tabular number at 74° 23′ will be 1.4623, and the logarithmic sine 8.8440, answering to 3° 58′; and at 86°, 2.0091, giving the logarithmic sine 9.3908, and the angular correction 14° 14′, so that the true situation of the ship will in this instance be more than a point further from the magnetic North than the compass indicates; it is also obvious that the correction will be very different from that which would be required, if the actual bearing of the ship's head were N.E. or N.W.

13. According to the observations collected and computed by the laborious and accurate Professor Hansteen, we have the actual intensity of the magnetic force in different places, as in the following table.

	Dip.	Intensity.
Peru - - -	0° 0′	1.0000
Mexico - - -	42 10	1.3155
Paris - - -	68 38	1.3482
London - -	70 33	1.4142
Christiania -	72 30	1.4959
Arendahl - -	72 45	1.4756
Brassa - - -	74 21	1.4941
Hare Island -	82 49	1.6939
Davis Straits -	83 8	1.6900
Baffin's Bay -	84 25	1.6685
	84 39	1.7349
	84 44	1.6943
	85 59½	1.7383
	86 9	1.7606

14. Notwithstanding the general agreement of this theory, with many of the observations made in the northern seas, it is still possible that some ships may have no *permanent* attraction; and there is reason to believe that the *induced magnetism* of the iron about a ship may not uncommonly have a perceptible effect on the compass; especially as it appears, from Mr. Barlow's experiments, that the guns are to be considered, with respect to magnetism, as soft or conducting. It will therefore be proper to inquire into some of the principal phenomena which may be induced from this cause.

15. If all the nails and bolts about the ship, together with the guns and ballast, were equally distributed in all possible directions, with respect to their longest dimensions, or even equally distributed into any three different directions perpendicular to each other, the effect on the needle would be very nearly the same as that of a single bar placed in the direction of the dipping needle, or of a sphere or shell of equivalent dimensions; so that it becomes interesting to inquire what would be the effect of such a sphere on the compass.

16. Supposing the sphere to be placed immediately before the compass, and on the same level with respect to the decks, the disturbing force would always completely vanish when the

ship's head pointed east or west; so that this is a case which may be excluded from further consideration.

17. In all other cases it may be shown that the needle, if otherwise at liberty, would be directed towards a point in the magnetic axis of the sphere at which it meets a plane, perpendicular to the line joining the sphere and the compass, and at one-third of the distance of the compass from the sphere. The direction of the force referred to the horizontal plane will be the projection of this direction, and its magnitude may be found from the relative latitude of the compass, with regard to the axis of the sphere (N. 6), requiring also to be reduced to the horizontal plane.

18. But for an easy and useful example of the result of such a calculation, it will be sufficient to take a case in which the primitive directive force of the sphere remains always horizontal, and the reductions are avoided. This will happen when the distance of the sphere before the compass, a, is to b, its depth below the compass, as $\sqrt{2}$ to 1; and when the ship's head is at the same time E. or W. Now the dip being D, the distance of the intersection of the axis with the plane already mentioned and with the horizontal plane, from the middle of the ship's breadth, will be b cot D, and the cotangent of the spontaneous deviation, $\frac{b}{a}$ cot D $= \sqrt{\frac{1}{2}}$ cot D, the tangent $\sqrt{2}$ ta D, the sine $= \sqrt{\frac{2ss}{1+ss}}$ and the cosine $\sqrt{\frac{1-ss}{1+ss}}$. The magnetic latitude λ, with respect to the sphere, will be such that $\sin\lambda = \frac{b\sin D}{c}$, c being $\sqrt{(a^2+b^2)} = \sqrt{(2b^2+b^2)} = \sqrt{3}b$ and $\sin\lambda = \sqrt{\frac{1}{3}}\sin D = \sqrt{\frac{1}{3}}\,.\,s$; consequently the sine of the dip ϵ with respect to the sphere is found $\frac{\sqrt{\frac{4}{3}}\,.\,s}{\sqrt{(1+ss)}} = \sqrt{\frac{2s}{\sqrt{(3+3ss)}}}$: and the magnetic force of the sphere, which varies as $\frac{\sin L}{\sin D}$ (N. 6) or here as $\frac{\sin\lambda}{\sin\epsilon}$ may be represented by $\sqrt{(1+ss)}$, considering the magnetism of the sphere as constant. But the magnetism of the sphere is proportional to that of the earth itself at the place of observation, so that the law of the composition of these forces is not affected by the change of their magnitude:

the direct force of the earth, however, requires to be reduced to the horizontal plane, while that of the sphere is already exerted in that plane. The direct force therefore may be called $\surd\,(1-ss)$ and the disturbance $f\,\surd\,(1+ss)$: which reduced to the direction of the magnetic meridian, becomes $f\surd(1+ss)\surd\frac{1-ss}{1+ss}=f\surd(1-ss)$ and to the transverse direction $f\surd(1+ss)\surd\frac{2ss}{1+ss}=f\surd 2, s$. The joint force in the direction of the meridian will therefore be always $(1+f)\,\surd\,(1-ss)$ and the transverse force $\surd\,2.fs$: consequently, the tangent of the angle of disturbance will be $\frac{\surd 2f}{1+f}\cdot\frac{s}{\surd(1-ss)}$, which is proportional to the tangent of the dip, and to that of the magnetic latitude.

19. Hence it appears that the tangent of the angular disturbance produced by the induced magnetism of a mass of iron so situated when the ship's head is E. or W., will vary as the tangent of the dip. It will also be in opposite directions on opposite sides of the magnetic equator. The disturbance on board the *Isabella*, in latitude 86°, if derived from this cause, would amount to 21° 19′; and it is remarkable, that conclusions, so nearly agreeing, should be derived from suppositions so totally different.

20. It is not improbable that the soft or conducting iron about a ship may often be so arranged as to produce effects considerably resembling those of a sphere or shell situated before the compass, and as much below it as is here supposed; but the proposition cannot be generally maintained that a sphere may *always* be so placed as to produce effects equivalent to those of the ship's magnetism, however the guns and ballast may be arranged. Supposing, indeed, the guns to constitute the principal part of the iron concerned, the deviation should vary initially in a ratio nearly approaching to that of the square of the sine of the apparent distance of the ship's head from the magnetic meridian, amounting to half of the maximum at about 45°, instead of about 30°, as it commonly appears to do; since the intensity of the induced magnetism of the guns would vary nearly as the simple sine of the distance, and its effect on

the compass again as the same sine. The deviation produced by the sphere would follow a very different law, but it is scarcely probable that this law would agree well enough with the results of observation, to make it necessary to investigate it here in a general manner. It is, however, obvious, that when the compass is in the plane of the magnetic equator of the sphere, the direction of the needle, as influenced by it, will be parallel to the magnetic axis of the sphere, and consequently in the magnetic meridian of the earth, so that the disturbance will disappear as it did in Mr. Barlow's experiments; and this circumstance, if it were ascertained by observation, would assist us in determining the place of the supposed sphere in the ship. But in the case here stated as an example of the situation of the sphere, the disturbance would never vanish, unless the dip were less than 54° 44′: the cosine of the angle formed by the ship with the meridian, when the force vanishes, being $\sqrt{\frac{1}{2}}$ the tangent of the dip: and this would happen first, in the northern hemisphere, when the ship's head pointed nearly south, while in the situation diametrically opposite, the disturbance would by no means vanish: so that the supposition of an induced magnetism, like that of a sphere, does not appear to be consistent with actual observation. Nor is it possible that a sphere should be so placed as to cause no disturbance whatever at the magnetic equator: and if the disturbance really vanishes at the equator, as has been asserted, it can only arise from an effect resembling that of the induced magnetism of a *vertical bar*.

No. XLI.

A BRIEF INVESTIGATION OF THE PROPERTIES OF THE

GEODETIC CURVE.

From Brande's Quarterly Journal for 1826, vol. xxi. p. 136.

PROFESSOR BESSEL has lately premised to his very elaborate and refined computations of latitudes and longitudes, on a spheroid, a demonstration of the elementary property of the curve of shortest distance, founded, as he says, on the theorem of *Taylor*, which affords him, for U', a value of U corresponding to $\varphi + z$, as a value of φ, z being a function of w, the expression " $U + \left(\frac{\mathrm{d}U}{\mathrm{d}\varphi}\right) z + \left(\frac{\mathrm{d}U}{\mathrm{d}p}\right)\frac{\mathrm{d}z}{\mathrm{d}w} + \ldots$" This may indeed be perfectly correct: but it would probably have surprised Dr. Brook Taylor not a little to see himself made responsible for such an inference: which it would have cost him much more labour to comprehend than it did to invent his theorem: and it would have staggered him most of all to see his finite increment "h" converted into a new flowing quantity z, and having a distinct fluxion assigned to it.

The true and natural method of solving these problems, and by far the simplest and most intelligible, is to use a separate notation for the variation of the curve, in its transition into another neighbouring curve: and, for a spheroid of rotation, the variation may be most conveniently supposed to be effected by the elementary removal of the points along the same parallels of latitude only, so that the curvature of the elements of latitude and longitude may remain unaltered.

Thus, if x be the angular latitude, y the longitude, and s the linear distance, R being the radius of the meridian, and r that of the parallel of latitude: we shall obviously have (1)

$ds^2 = R^2 \, dx^2 + r^2 \, dy^2$; and hence, following exactly the steps of the *Elementary Illustrations of Laplace*, No. 289, Sch. 2, P. 152, we have $\delta ds = \frac{r r dy \delta dy}{ds}$, since $\delta x = 0$; and since $d\left(\frac{r^2 dy}{ds} \delta y\right) = \frac{r^2 dy}{ds} d\delta y + d \frac{r^2 dy}{ds} \delta y = \frac{r^2 dy}{ds} \delta dy + d \frac{r^2 dy}{ds} \delta y$, we have $\int \delta ds = r^2 \frac{dy}{ds} \delta y - \int d \frac{r^2 dy}{ds} \delta y = \delta s$. Now in order that the distance may be the shortest possible, this fluent, taken between the extreme points of the curve, must vanish; and at each of these points the variation δy must wholly vanish, although it is supposed at the intermediate points to have a value comparable to the other varying elements: consequently the second part of the fluent, $\int d \frac{rrdy}{ds} \delta y$, must be every where $= 0$, since it cannot have alternately positive and negative values, consistently with the required property of the curve, which must everywhere be the shortest possible: and this can only happen when $d \frac{rrdy}{ds} = 0$, and $r^2 \frac{dy}{ds}$ is a constant quantity. But $\frac{rdy}{ds}$ is the sine of the inclination of the curve to the meridian, which must therefore be inversely as r the radius of the parallel of latitude, in order that $r \frac{rdy}{ds}$ may be constant. Mr. Fog Thune, assisted by Professor Bessel, has investigated the curve more generally, by the method of variations, in his Spheroidical Trigonometry, without limiting it in the first instance to a spheroid of rotation. We may now proceed to Professor Bessel's latest computations.

This investigation is followed by a translation of Bessel's formulæ and computations for geographical latitudes and longitudes on a spheroid.—*Note by the Editor.*

No. XLII.

A SIMPLE RECTIFICATION OF THE

GEODETIC CURVE.

From Brande's Quarterly Journal for 1826, vol. xxi. p. 153.

Professor Bessel's investigation of the properties of the geodetic curve, though most ingenious and successful, is yet so intricate and complicated, that it is easier to obtain a new solution of the problem, than to verify the steps of his researches in such a manner as to fulfil the whole duty of a scientific translator. For obtaining this solution it is only necessary to set out in a right direction; and considering the dependence of the curve on the distance from the axis, r, it is natural to inquire whether its properties may not be most conveniently expressed in terms of that quantity. The equation of the ellipsis being $\frac{xx}{aa} + \frac{yy}{bb} = 1$, and x being the r of this investigation, we have $\frac{rr}{aa} + \frac{yy}{bb} = 1$, $\frac{r\mathrm{d}r}{aa} + \frac{y\mathrm{d}y}{bb} = 0$; and the square of the fluxion of the arc of the meridian, which is equal to $\mathrm{d}r^2 + \mathrm{d}y^2$, $\mathrm{d}y$ being $= -\frac{bb}{aa}\frac{r}{y}\mathrm{d}r$, becomes $\mathrm{d}r^2 + \frac{b^4}{a^4}\frac{rr}{yy}\mathrm{d}r^2$, or since $y^2 = b^2 - \frac{bb}{aa}r^2$, $\mathrm{d}r^2\left(1 + \frac{b^4}{a^4} - \frac{a^2r^2}{a^2b^2 - b^2r^2}\right) = \mathrm{d}r^2\left(1 + \frac{b^2r^2}{a^4 - a^2r^2}\right)$; and making the least value of r equal to g, we have every where $\frac{g}{r}$ for the sine of the azimuth, and for the square of its cosine $1 - \frac{gg}{rr} = \frac{rr - gg}{rr}$. Hence $\mathrm{d}s^2 = \mathrm{d}r^2\frac{rr}{r - gg}\left(1 + \frac{b^2r^2}{a^4 - a^2r^2}\right)$. Making now $r^2 - g^2 = \gamma^2$, we have $r\mathrm{d}r = \gamma\mathrm{d}\gamma$, $\mathrm{d}r = \frac{\gamma\mathrm{d}\gamma}{r}$, $\mathrm{d}r^2 = \frac{\gamma\gamma}{rr}\mathrm{d}\gamma^2$, and $\mathrm{d}s^2 = \mathrm{d}\gamma^2\left(1 + \frac{b^2r^2}{a^4 - a^2r^2}\right) = \mathrm{d}\gamma^2\frac{a^4 - (a^2 - b^2)r^2}{a^4 - a^2r^2}$; or, if $a^2b^2 = e^2$, $\mathrm{d}s^2 = \mathrm{d}\gamma^2\frac{a^4 - e^2r^2}{a^4 - a^2r^2}$, and $\mathrm{d}s = \mathrm{d}\gamma\sqrt{(a^4 - e^2g^2 - e^2\gamma^2)}$

$\sqrt{\frac{1}{a^4-a^2g^2-a^2\gamma^2}} = d\gamma \sqrt{(a^4-e^2g^2)} \sqrt{(1-\frac{e^2}{a^4-e^2g^2}\gamma^2)} \sqrt{\frac{1}{a^4-a^2g^2}}$ $\sqrt{\frac{1}{1-\frac{\gamma\gamma}{aa-gg}}} = d\gamma \sqrt{(a^4-e^2g^2)} \sqrt{(1-f^2\chi^2)} \sqrt{\frac{1}{a^4-a^2g^2}}$ $\sqrt{\frac{1}{1-\chi\chi}}$; χ^2 being $= \frac{\gamma\gamma}{aa-gg}$, $f^2\chi^2 = \frac{e^2}{a^4-e^2g^2}\gamma^2$, or $f^2 = \frac{aa-gg}{a^4-e\,g^2}e^2$; so that $d\chi = \frac{d\gamma}{\sqrt{(aa-gg)}}$; whence $ds = d\chi\sqrt{(aa-gg)}$ $\sqrt{\frac{a^4-e^2g^2}{a^4-a^2g^2}} \sqrt{(1-f^2\chi^2)} \sqrt{\frac{1}{1-\chi\chi}} = \sqrt{(a^2-\frac{ee}{aa}g^2)} \frac{d\chi}{\sqrt{(1-\chi\chi)}}$ $(1-\frac{1}{2}f^2\chi^2+\frac{1}{8}f^4\chi^4-\frac{1}{16}f^6\chi^6+\frac{5}{128}f^8\chi^8-\ldots)$. The fluents may be found by Hirsch's Tables, p. 143; and we shall have $s = \sqrt{(a^2-\frac{ee}{aa}g^2)}\,(P-\frac{1}{2}f^2\,Q+\frac{1}{8}f^4\,R-\frac{1}{16}f^6\,S+\ldots)$; P being arc sin χ, $Q = \frac{P-\chi\cos. P}{2}$, $R = \frac{3Q-\chi^3\cos. P}{4}$, $S = \frac{5R-\chi^5\cos. P}{6}$, $T = \frac{7S-\chi^7\cos. P}{8}$, and so forth.

It will be convenient to have the values of these fluents expressed in a table, for different values of χ, or $\sqrt{\frac{rr-gg}{aa-gg}}$, from 0 to 1; such a table will probably be inserted in the next number of this Journal; and it may also be useful on many other occasions, giving, for example, the length of an elliptical arc, when $g = 0$, $\chi = \frac{r}{a}$, f being then $= \frac{e}{a}$.

The value of r may be readily found from the latitude and the ellipticity: for t being the tangent of the latitude, we have $t = \frac{dr}{dy} = \frac{aa}{bb}\frac{y}{r}$; $t^2 = \frac{a^4}{b^4}.\frac{yy}{rr}$, $y^2 = b^2 - \frac{bb}{aa}r^2$; $t^2 = \frac{a^4}{b^4}.\frac{a^2b^2-b^2r^2}{a^2r^2} = \frac{aa}{bb}.\frac{aa-rr}{rr}$; $\frac{bb}{aa}r^2t^2 = a^2-r^2$; $\left(\frac{bb}{aa}t^2+1\right)r^2 = a^2$, and $r^2 = \frac{a^4}{bbtt+aa} = \frac{a^2}{\frac{bb}{aa}t^2+1}$; and if we made $a = 1$, and $b = 1-\varepsilon$, $r^2 = \frac{1}{1+tt-2\varepsilon tt+\varepsilon\varepsilon tt}$; or, if $\varepsilon = \frac{1}{300}$, $r^2 = \frac{1}{1+\frac{149}{150}tt}$, very nearly.

Having found the values of r, for any two latitudes, by this formula, we can easily compute the corresponding value of s;

and if s is given by measurement, we can correct the supposed value r from the error of s, and the azimuth of the curve, the sine of which is always $\frac{g}{r}$.

In order to find the angular difference of longitude, we must multiply the fluxion ds by the sine $\frac{g}{r}$, and divide it by the radius r, and we have $\frac{g\mathrm{d}s}{rr} = \frac{g}{rr}\sqrt{\frac{rr}{rr-gg}}\sqrt{\frac{a^4-e^2r^2}{a^4-a^2r^2}}\,\mathrm{d}r = \frac{g\mathrm{d}r}{r}$ $\sqrt{\frac{a^4-e^2r^2}{(a^4+a^2g^2)r^2-a^2r^4-a^4g^2}} = \frac{g\mathrm{d}(r^2)}{2rr}\sqrt{\frac{a^4-e^2r^2}{(a^4+a^2g^2)r^2-a^2r^4-a^4g^2}}$; of which the fluent may be found by comparison with the $\int\frac{x^m\mathrm{d}x}{\sqrt{(a+bx+cx^2)}}$ of Hirsch, p. 187, 183, 185; Suppl. Enc. Brit., Art. FLUENTS, No. 353; or perhaps more conveniently by multiplying the series already found into the development of $\frac{1}{rr} = \frac{1}{gg+\gamma\gamma} = \frac{1}{gg}\cdot\frac{1}{1+\frac{\gamma\gamma}{gg}} = \frac{1}{gg}\left(1-\frac{\gamma\gamma}{gg}+\frac{\gamma^4}{g^4}-\frac{\gamma^6}{g^6}+\ldots\right)$; and this will be sufficiently convergent when γ is small; when larger, the direct computation of the fluents will be required, which is necessarily a little more tedious, as it cannot be materially assisted by a table; and this may possibly have been the reason that Professor Bessel has employed a different mode of investigation.

In page 337 of the same volume of the Quarterly Journal is given 'A Table of Coefficients subservient to Geodetical Computations.' The Table of Fluents formed as recommended in the text, was found to be very voluminous, and therefore not inserted, exhibiting a striking contrast to Bessel's tables, both in simplicity and facility of application.—*Note by the Editor.*

No. XLIII.

CALCULATION OF THE RATE OF EXPANSION OF A SUPPOSED

LUNAR ATMOSPHERE.*

From Nicholson's Journal of Natural Philosophy and Chemistry for 1808, vol. xx. p. 117.

To Mr. Nicholson.

Sir,

It has been a subject of inquiry among some who are attached to astronomical speculations, whether or no, if the moon had ever been possessed of an atmosphere equally dense with that of the Earth, she could have retained it, without a very sensible diminution, in consequence of the Earth's attraction, upon the supposition of the infinite dilatability of the air, with a density always proportional to the pressure. The inquiry involves a great variety of considerations, and it would be extremely difficult to make an exact calculation of all the particulars connected with it; but it may be shown from some general principles that the diminution would have become perceptible to a spectator situated on the Earth, in the course of a few centuries.

If a be the distance of the moon from the earth, and x the distance of any other point in the line joining them, the force of gravitation will be as $\frac{1}{x^2} - \frac{1}{70\,(a-x)^2}$; and the centrifugal force, arising from the revolution round the common centre of gravity, to be added for the terrestrial atmosphere,

* This article and the two which follow are inserted as examples of the brevity and apparent simplicity of Dr. Young's mode of dealing with very difficult problems: unfortunately they form no exceptions to the extreme obscurity which usually characterizes his applications of mathematics to physical inquiries, more especially in early life, when he had little knowledge of, and no respect for, the forms of modern analysis.—*Note by the Editor.*

and to be subtracted for the lunar, being equal to the force of gravitation at the distance of the centres, the joint force f acting on the particles of the atmosphere will be as $\frac{1}{x^2}-\frac{1}{70(a-x)^2}+\frac{1}{70a^2}$, and $\frac{1}{x^2}-\frac{1}{70(a-x)^2}-\frac{1}{a^2}$ respectively: or, since f must be equal to unity at the earth's surface, when x is equal to the earth's semidiameter b, $f=\frac{b^2}{x^2}$ near the earth, without sensible error, and $f=\frac{b^2}{x^2}-\frac{b^2}{70(a-x)^2}-\frac{b^2}{a^2}$, for the lunar atmosphere. Then the density being y, which may also be called unity at the earth's surface, we have $-c\dot{y}=fy\dot{x}$, and it is obvious that c must express the height of a column of air of uniform density capable of producing the pressure by its weight, in order that $-c\dot{y}$ may be initially equal to $\dot{x}$. Hence we have H . L . $\frac{1}{y}=\frac{1}{c}\cdot\int f\dot{x}$; but $f\dot{x}=b^2\left(\frac{\dot{x}}{x^2}-\frac{\dot{x}}{70(a-x)^2}-\frac{\dot{x}}{a^2}\right)$: therefore H . L . $\frac{1}{y}=\frac{b}{c}\left(d-\frac{b}{x}-\frac{b}{70(a-x)}-\frac{bx}{a^2}\right)$, d being, without sensible error, $1+\frac{b^2}{a^2}$. Now b is 3958, and c 5.28 miles, and at the moon's surface x is about $60b$, and $a-x=\frac{3}{11}b$; whence H.L. $\frac{1}{y}=685.69$. Again, when f vanishes, and the density is least, $\frac{1}{x^2}=\frac{1}{70(a-x)^2}+\frac{1}{a^2}$, and x is nearly .825 a, whence H . L . $\frac{1}{y}=724.31$; and this density is to the density at the moon's surface as 1 to the number of which the hyperbolic logarithm is 38.62, and the common logarithm 16.773: and supposing the density to be increased in any given ratio, the proportion will remain the same, the number c still indicating the height of a column equal in density to the atmosphere, thus condensed, at the earth's surface.

Now the expansion of the lunar atmosphere, supposing it to be equal in density to that of the Earth, and to extend to the point where the force f vanishes, which is the most favourable condition for its permanence, may be determined from this general principle; that the motion of the centre of gravity of any system of bodies, some of which are urged by a greater force in

one direction than in another, must be the same as if the difference of the forces acted on the whole system collected into the centre of gravity. Thus, if the pressure of the highly rarefied air, at the termination of the supposed lunar atmosphere, which would have kept it in equilibrium, be removed, the elasticity of the column pressing on the moon will be by so much greater than its gravitation; and the centre of gravity of the column will be repelled, with a velocity as much smaller than that of a body falling at the Earth's surface, as the pressure removed is smaller than the weight of the column: but this ratio is compounded of that of the densities at the opposite ends of the column, and that of the force of gravitation, or rather the force f, near the moon's surface, to its force at the surface of the Earth, since the mass required to produce the given density, by its pressure, is as much greater, as the gravitation is smaller; and if we diminish in this proportion the space which a falling body would describe in a century, we shall have 514 feet for the elevation of the centre of gravity of a column of the lunar atmosphere in that time.

But in order to estimate the effect of such a change, we must calculate the actual height of the centre of gravity of a given column of an elastic fluid: and for this purpose we may suppose the attractive force uniform. The height of the centre of gravity is determined by dividing the fluent of $xy\dot{x}$ by the mass, or by $1-y$; but since $-c\dot{y}=y\dot{x}$, $xy\dot{x}=-cx\dot{y}$, x being $=c$ (H. L. $\frac{1}{y}$), or according to a mode of expression lately employed by one of your correspondents, $cm\,(y^{-\frac{1}{m}}-1)$, when m is infinite; hence $-cx\dot{y}=ccm\,(\dot{y}-y^{-\frac{1}{m}}\dot{y})$, of which the fluent is $e+ccm\,(y-\frac{1}{1-\frac{1}{m}}y^{1-\frac{1}{m}})=e-cxy-cy^{1-\frac{1}{m}}$, or $e-cxy-cy$; which must vanish when $y=1$ and $x=0$; consequently $e=c$, and the height of the centre of gravity is $c-\frac{cxy}{1-y}$; and when $y=0$, this height is equal to that of the column c, which for the earth's atmosphere is 5.28 miles, and for the moon's as much greater as the force is smaller, that is 27.75 miles. The centre of gravity being therefore elevated 514 feet, or $\frac{1}{285}$ of its height, in a cen-

tury, the mean density of the column must also be reduced about $\frac{1}{300}$; but since a certain part of this elevation depends on the supposed acceleration of the more distant portions, which would produce no sensible effect in the neighbourhood of the moon, we cannot estimate the mean rarefaction of the part remaining more nearly in its original situation, at more than about $\frac{1}{300}$; and this will be reduced to about one fourth for the mean of the whole atmosphere, surrounding the moon on all sides: so that we may take $\frac{1}{1200}$ for the mean rarefaction of such a lunar atmosphere in the course of the first century.

So small a rarefaction as this would certainly not be directly observable at the distance of the Earth. Supposing that the atmosphere would be visible until its density became equal to a given quantity, the point, at which this density would be found, would be depressed only about 18 miles, if the whole density of the atmosphere were reduced to one half, and by a diminution of $\frac{1}{1200}$, only $\frac{1}{1200}$ of 27.75 miles, or about 120 feet. The effect of an atmosphere would however be more perceptible in the refraction, which would occasion an alteration in the apparent place of a star about to be eclipsed, and which would amount, in the case of the Earth's atmosphere, to 66 minutes. But the refractive density of the lunar atmosphere would vary nearly as the 134th root of the distance, instead of the 7th; and the deviation, instead of 66 minutes, would become 13' 50", one 1200th of which would be only $\frac{7}{10}$ of a second, which would still be imperceptible; although in two or three centuries, since the rarefaction would increase at first as the square of the time, it might perhaps be discoverable; and this would be considerably sooner than the decrease of the moon's apparent diameter could be observed. It is however scarcely probable, that so slow a rate of diminution could have reduced the lunar atmosphere from a density equal to that of the terrestrial atmosphere, to its present state, in the course of 10,000 years.

I am, Sir,
Your very obedient servant,
HEMEROBIUS.

16 May, 1808.

No. XLIV.

A CONCISE METHOD OF DETERMINING THE

FIGURE OF A GRAVITATING BODY

REVOLVING ROUND ANOTHER.

From Nicholson's Journal for 1808, vol. xx. p. 208.

To Mr. NICHOLSON.

SIR,

IT is well known that there are some imperfections in Sir Isaac Newton's investigations respecting the figures of gravitating bodies, which have been supplied by Maclaurin and Clairaut: the subject is however still considered as difficult and intricate, and the simplest calculations respecting it have hitherto been too prolix, to be distinctly conceived as links of the same chain. I shall endeavour to point out a method of treating it which is extremely compendious and convenient.

Neglecting in the first place the diurnal rotation, we may suppose that each particle of the body revolves in an equal orbit, so that its centrifugal force may be equal to the mean attractive force; then the local attractive force will be greater or less than this by a difference which must obviously be proportional to the distance from an equatorial plane perpendicular to the direction of the central body, and tending to remove the body from this plane. A second disturbing force will also arise from the want of parallelism in the direction of the attractive force, which will tend towards the line joining the centres, and will be every where to the whole force as the distance from this line to the distance of the bodies. Now if each of these forces be reduced to the direction of the circumference of the sphere, from which the figure is supposed to vary but very little, it will

be every where proportional to the product of the sine and cosine of the distance from the equatorial plane, and when this distance is half a right angle, each of them will be half as great as in its entire state. Thus the gravitation towards the moon at the earth's surface is to the gravitation towards the earth as 1 to 70 times the square of 60½, or to 256217, and the first disturbing force is to the whole of this as 2 to 60½, at the point nearest to the moon, and the second as 1 to 60½ at the equatorial plane; and the sum of both reduced to the direction of the circumference where greatest, as 3 to 121, that is, to the whole force of the earth's gravitation as 1 to 10,334,000. And in a similar manner the joint disturbing force of the sun is to the weight as 1 to 25,736,000.

Now if a sphere be inscribed in an oblong spheroid, the elevation of the spheroid above the sphere must obviously be proportional, if measured in a direction parallel to the axis of the spheroid, to the ordinate of the sphere, that is, to the sine of the distance from its equator; and if reduced to a direction perpendicular to the surface of the sphere, it must be proportional to the square of that sine; and the tangent of the inclination to the surface of the sphere, which is as the fluxion of the elevation divided by that of the circumference, must be expressed by twice the continual product of the sine, the cosine, and the ellipticity or greatest elevation, the radius being considered as unity: so that the ellipticity will also express the tangent of the inclination where it is greatest; and the inclination will be every where as the product of the sine and cosine.

If therefore the density of the elevated parts be considered as evanescent and their attraction be neglected, there will be an equilibrium when the ellipticity is to the radius as the disturbing force to the whole force of gravitation: for each particle situated on the surface will be actuated by a force precisely equal and contrary to that which urges it in the direction of the inclined surface. Hence, if the density of the sea be supposed inconsiderable in comparison with that of the earth, the radius being 20,839,000 feet, the height of a solar tide in equilibrium will be 2.0166 feet, and that of a lunar tide .8097.

We must next inquire what will be the effect of the gravitation of the elevated parts, on any given supposition respecting

their density. Let us imagine the surface to be divided by an infinite number of parallel and equidistant circles, beginning from any point at which a gravitating particle is situated, and let their circles be divided by a plane bisecting the equatorial plane of the spheroid ; it is obvious that if the elevations on the opposite sides of this plane be equal in each circle, no lateral force will be produced; but when they are unequal, the excess of the matter on one side above the matter on the other will produce a disturbing force. The elevation being every where as the square of the distance from the equatorial plane, the difference, corresponding to any point of that semicircle in which the elevation is the greater, will be as the difference of the squares of the distances of the corresponding points of the two semicircles, that is, as the product of the sum and the difference of the distances: but the sum is twice the distance of the centre of the circle from the equatorial plane, or twice the sine of the distance of the gravitating particle from the plane, reduced in the ratio of the radius to the cosine of the angular distance of the circle from its pole ; and the difference is twice the actual sine of any arc of the circle, reduced to a direction perpendicular to that of the plane, that is, reduced in the proportion of the radius to the cosine of the angular distance of the given particle from the equatorial plane. From these proportions it follows, that, in different positions of the gravitating particle, the effective elevation at each point of the surface, similarly situated with respect to it, is as the product of the sine and cosine of its angular distance from the equatorial plane, the other quantities concerned remaining the same in all positions: the disturbing attraction of all the prominent parts varies therefore precisely in this ratio, the matter which produces it being always similarly arranged, and varying only in quantity; consequently the sum of this attraction and the original disturbing force both vary as the inclination of the surface, and may be in equilibrium with the tendency to descend towards the centre, provided that the ellipticity be duly commensurate to the density of the elevated parts.

In the last place we must investigate what is the magnitude of the ellipticity corresponding to a given disturbing force and a given density. It follows from the proportions already mentioned,

first, that the effectual elevation at each point of each concentric semicircle is proportional to the sine of its distance from the bisecting plane; and secondly, that the greatest effective elevation of each semicircle, for any one position of the superficial particle, is as the product of the sine and the cosine of the angular distance from that particle, the diameter of the circle being as the sine, and the distance of its centre from the equatorial plane as the cosine. It may easily be shown, that the disturbing force, reduced to the direction of the surface, or of the plane of each circle, is equal to the attraction which would be exerted by the matter covering the whole semicircle to a height equal to half the greatest elevation, if placed at the middle point: for the elevation being as the sine of the distance from the bisecting plane, and the comparative effect being also as the sine, the attraction for each equal particle of the semicircle is as the square of the sine, and the whole sum half as great as if each particle produced an equal effect with that on which the elevation is greatest. We must therefore compute the attraction of the quantity of matter thus determined, supposing it to be disposed at the respective points of a great circle passing through the given point and the pole of the spheroid. The immediate attraction of each particle being inversely as the square of the chord, its effect reduced to the common direction will be as the sine directly, and the cube of the chord inversely, and this ratio being compounded with that of the product of the cosine and the square of the sine, which expresses the quantity of matter at each point, the comparative effect will be as the cube of the sine and the cosine directly, and as the cube of the chord inversely, or as the cube of the cosine of half the arc and the cosine of the whole arc conjointly. If therefore we call the cosine of half the arc x, the cosine of the whole arc will be $2x^2 - 1$, and the fluxion of the arc being $-\frac{2\dot{x}}{\sqrt{(1-xx)}}$, that of the force will be $\frac{2x^3\dot{x} - 4x^5\dot{x}}{\sqrt{(1-xx)}}$, of which the fluent is $(\frac{4}{5}x^4 + \frac{2}{5}x^2 + \frac{4}{5})\sqrt{(1-xx)}$, as may be shown by substituting, in the reduction of its fluxion, $\frac{1-xx}{\sqrt{(1-xx)}}$ for $\sqrt{(1-xx)}$: and while x decreases

from 1 to 0, this fluent becomes $\frac{1}{4}$. But in order to determine the unit with which this quantity is to be compared, we must consider the initial force as unity, and imagine that it is continued through an arc equal in length to the radius; and we must find the attraction of the solid contained between a circular plane and a conical surface, initially touching the effective portion of the elevation, and including it between them; the attraction reduced to a common direction, being initially half the whole attractive force of such a solid, as we have already seen of the concentric circles considered separately. But the attraction of any slender conical or pyramidical body for a particle placed at its vertex, is three times as great as that of the same quantity of matter situated at its base; consequently the attraction of the supposed solid is equal to that of the circumscribing semicylinder placed at the distance of the radius: the conical excavation being half of the solid, and the semicylinder triple of the cone: but the height of this semicylinder in the case of a particle situated half way between the pole and the equator of the spheroid is twice the ellipticity, the tangent of the angle of mutual inclination of the surfaces of the effective elevation being initially equal to twice the greatest ordinate, because the product of the sine and cosine, when greatest, is equal to half of the radius: the semicylinder will therefore be equal to a cylinder of which the diameter is equal to that of the sphere, and the height equal to the ellipticity; and the contents of this cylinder will be to that of the sphere, as $\frac{3}{2}$ of the ellipticity to the radius. Such therefore is the unit with which the disturbing attraction is to be compared; and when the densities are equal, this force will be to the whole weight as $\frac{3}{8}$, $\frac{1}{5}$, or $\frac{3}{5}$ of the ellipticity to the radius; and the portion of the inclination remaining to be compensated by the primitive disturbing force will be $\frac{3}{5}$ of the whole, so that the ellipticity must be to the proportional disturbing force as 5 to 2. And if the density of the sea be to the mean density of the earth as 1 to n, the disturbing force, produced by its attraction, will be to the ellipticity as $\frac{3}{5n}$ to 1, and the primitive disturbing force as $1 - \frac{3}{5n}$ to 1.

The heights of the solar and lunar tides in equilibrium having been found equal to .8097 and 2.0166 feet respectively, on the supposition of the density of the sea being inconsiderable, they must be increased to 2.024 and 5.042 for an imaginary planet of uniform density; but since n is in reality about $5\frac{1}{2}$, and $\frac{3}{5n}$ nearly $\frac{1}{9}$, the ellipticity must be to the primitive disturbing force only as 1 to $\frac{8}{9}$ or 9 to 8, and the height of the tides in equilibrium .911 and 2.269 respectively, and the joint height 3.18 feet. And when the surface assumes any other form than that which affords the equilibrium, the force tending to restore that form is always less by one ninth than it appears to be when the attraction of the elevated parts is neglected. The theory of the tides must therefore be very materially modified by these considerations, although they do not affect the general method of explaining the phenomena.

These calculations are also immediately applicable to the figure of an oblate spheroid: for it may easily be shown, that the difference of the elevations in the opposite halves of each semicircle is precisely the same in an oblate as in an oblong spheroid of equal ellipticity: so that the ellipticity must here also be to the disturbing force, where it is greatest, as 1 to $1-\frac{3}{5n}$, or to the centrifugal force at the equator as 1 to $2-\frac{6}{5n}$. Thus, the centrifugal force being $\frac{1}{289}$, if the density were uniform, the ellipticity would be $\frac{1}{231}$; but since it is in reality about $\frac{1}{315}$, $2-\frac{6}{5n}=\frac{315}{289}$, and $n=1.32$, n implying here the mean density of the earth compared with the mean density of the elevated portion of the spheroid, which hence appears to be about three fourths of that of the whole earth. It is obvious that, in this case as well as in the former, if the density of the sea were two thirds greater than that of the earth, the slightest disturbing force would completely destroy the equilibrium, and the whole ocean would be collected on one side of the earth.

I am, Sir,
Your very humble servant,
A. B. C. D.

No. XLV.

CALCULATION OF THE DIRECT

ATTRACTION OF A SPHEROID,

AND DEMONSTRATION OF CLAIRAUT'S THEOREM.

From Nicholson's Journal for 1808, vol. xx. p. 273.

To Mr. Nicholson.

Sir,

The same mode of calculation, by which the figure of a gravitating body, differing but little from a sphere, has been determined (No. XLIV.), is also applicable to the magnitude of its immediate attraction, or the comparative length of a pendulum in different latitudes.

Suppose a sphere to be inscribed in the spheroid, and another to be circumscribed about it; I shall first show that the attraction at the pole is equal to that of the smaller sphere increased by $\frac{4}{15}$ of that of the shell, and at the equator equal to that of the larger diminished by $\frac{2}{15}$. If we call the attraction of this shell 2, its surface being equal to the curved surface of a circumscribing cylinder, the attraction of a narrow ring of this cylinder, or of the elevated portion of the spheroid at the equator, supposed to act at the distance of the radius, or unity, may be expressed by its breadth; but in its actual situation its attraction in the direction of the axis is reduced in the ratio of the cube of the chord of half a right angle to the cube of the radius; and the attraction of any other ring will be to this in the ratio of the quantity of matter, or the cube of the sine of the distance from the pole, and of the versed sine directly, and in the ratio of the cube of the chord inversely; that is in the joint ratio of the cube of the cosine of half the angle and the versed sine: thus, if we call the cosine of half the angle x, the versed

sine being $2-2x^2$, and the fluxion of the arc $\frac{2\dot{x}}{\sqrt{(1-xx)}}$, the fluxion of the force at the equator will be $\frac{1}{2\sqrt{2}} \cdot \frac{2\dot{x}}{\sqrt{(1-xx)}}$, and elsewhere as much less as $x^2\ (2-2x^2)$ is less than $\frac{1}{2\sqrt{2}}$, that is, $\frac{4x^2\ \dot{x}}{\sqrt{(1-xx)}} - \frac{4x^5\ \dot{x}}{\sqrt{(1-xx)}}$, of which the fluent is found as before $(\frac{4}{5} x^4 - \frac{4}{15} x^2 - \frac{8}{15}) \sqrt{(1-xx)}$; and this becomes $\frac{8}{15}$ while x increases from 0 to 1, being to 2, the attraction of the whole shell, as $\frac{4}{15}$ to 1; but if the radius of the sphere be 1 and the ellipticity e, the attraction of the shell will be to that of the sphere as $\frac{3e}{n}$ to 1, n being the mean density of the sphere, compared with that of the superficial parts, and the attraction of the spheroidal prominence will be expressed by $\frac{4}{5}\frac{e}{n}$, that of the sphere being unity.

The depression below the circumscribed sphere is equal, on the meridian, to the elevation above the inscribed sphere; but vanishes at the equator, being every where proportional to the square of the sine of the latitude; so that the mean depression of each of an infinite number of rings, of which any point of the equator is the pole, must be half as great as the elevation of the corresponding rings parallel to the equator; and the whole deficiency is equal to half of the whole excess, that is, to $\frac{2e}{5n}$; consequently the remaining attraction of the shell is $\frac{13e}{5n}$, from which we must deduct the diminution of the attraction of the inscribed sphere $2e$, and the whole will become $1 + \frac{13e}{5n} - 2e$, which subtracted from $1+\frac{4e}{5n}$ leaves $2e - \frac{9}{5}\frac{e}{n}$ for the excess of the immediate attraction at the pole above the equatorial attraction; to which if we add the centrifugal force f, the whole diminution of gravity g will be $2e - \frac{9e}{5n} + f$; but since e was before found to f as 1 to $2 - \frac{6}{5n}$ or $= \frac{5n}{10n-6} \cdot f$, we have $\frac{10n-9}{5n} \cdot e = \frac{10n-9}{10n-6} \cdot f$, and $g = \frac{20n-15}{10n-6} \cdot f$, to which if we add e, we find $e+g = \frac{25n-15}{10n-6} \cdot f = \frac{5}{2} f$; and this is the celebrated theorem of Clairaut.

It remains to be shown, that the diminution of the attractive force at different parts of the spheroid varies as the square of the cosine of the latitude. The elevation, being every where proportional to the square of the distance from the axis, may be divided into two parts; one proportional to the square of the sine of the distance from the meridian of the place, and the other to the distance from the plane of another meridian perpendicular to it: but the first of these being constant, whatever may be the position of the place to be considered, the second only produces the variation. Now if we take in the second portion the mean of the elevations at any two points of a less circle equidistant from the meridian, it will be proportional to the sum of the squares of the distance of the centre of the circle from the axis, and of the cosine of the distance from the meridian in the same circle, reduced to a similar direction, that is, diminished in the ratio of the radius to the sine of the latitude, since twice the sum of the squares of any two quantities is equal to the sum of the squares of their sum and their difference. We have therefore two quantities, varying as the square of the cosine and as the square of the sine of the latitude respectively: but the square of the sine may be represented by a constant quantity diminished by the square of the cosine: and the decrease of the attraction of the inscribed sphere is as the elevation, which is as the square of the cosine; the centrifugal force reduced to a vertical direction is also as the square of the cosine. We have therefore, beside two constant quantities, two negative forces and a positive one, all varying as the squares of the cosine of the latitude; and it is obvious, that the joint result of the whole, or the upper real diminution of gravity, must also vary in the same proportion.

A. B. C. D.

29 June, 1808.

No. XLVI.

ON THE EQUILIBRIUM AND STRENGTH OF

ELASTIC SUBSTANCES.

From the Mathematical Elements of Natural Philosophy, in the second volume of Dr. Young's Lectures, sect. ix. p. 46.*

1. Definition. A substance perfectly elastic is initially extended and compressed in equal degrees by equal forces, and proportionally by proportional forces.

2. Definition. The modulus of the elasticity of any substance is a column of the same substance, capable of producing a pressure on its base which is to the weight causing a certain degree of compression, as the length of the substance is to the diminution of its length.

3. Theorem. When a force is applied to an elastic column, of a rectangular prismatic form, in a direction parallel to the axis, the parts nearest to the line of direction of the force exert a resistance in an opposite direction; those particles, which are at a distance beyond the axis, equal to a third proportional to the depth and twelve times the distance of the line of direction of the force, remain in their natural state; and the parts beyond them act in the direction of the force.

The forces of repulsion and cohesion are initially proportional to the compression or extension of the strata, and these to their distance from the point of indifference: the forces may therefore be represented by the weight of two triangles, formed by the intersection of two lines in the point of indifference; and their actions may be considered as concen-

* This article has been reprinted in consequence of the originality and importance of some of the propositions which it contains. It was not included in the new edition of Dr. Young's Lectures, which was edited by Professor Kelland.—*Note by the Editor.*

trated in the centres of gravity of the triangles, which are at the distance of two thirds of the length of each from the vertex, and at the distance of two thirds of the depth from each other. This distance constitutes one arm of a lever, which is of constant length, while the distance of the line of direction of the force from the centre of gravity of the nearest triangle constitutes the other arm; and calling the distance of the line of direction of the force from the axis, a, and the depth b, the length of this arm, on the supposition that the point of indifference is at the assigned distance, will be $a+\frac{bb}{12a}-\frac{2}{3}\left(\frac{1}{2}b+\frac{bb}{12a}\right)$, or $a+\frac{bb}{36a}-\frac{1}{3}b$, that of the constant arm being $\frac{2}{3}b$. The cohesive and repulsive forces must therefore be as $a+\frac{bb}{36a}-\frac{1}{3}b$ to $a+\frac{bb}{36a}+\frac{1}{3}b$, since that which serves as the fulcrum of the lever must bear a force equal to the sum of the two forces applied at the ends, which are proportional to the opposite arms of the lever; or as $36aa-12ab+bb$ to $36aa+12ab+bb$, that is, as $(6a-b)^2$ to $(6a+b)^2$: but these forces are actually as the squares of the sides of the similar triangles which represent them, that is, as $\left(\frac{1}{2}b-\frac{bb}{12a}\right)^2$ to $\left(\frac{1}{2}b+\frac{bb}{12a}\right)^2$, or as $(6a-b)^2$ to $(6a+b)^2$, which is the ratio required: there will therefore be an equilibrium under the circumstances of the proposition.

4. Theorem. **The weight of the modulus of the elasticity of a column being m, a weight bending it in any manner f, the distance of the line of its application from any point of the axis a, and the depth of the column b, the radius of curvature will be $\frac{bbm}{12af}$.**

Supposing first the force to act longitudinally, and $a=\frac{1}{6}b$, the point of indifference will be in the remoter surface of the column, and the compression or extension of the nearer surface will be twice as great as if the force had been applied equally to all the strata; and will therefore be to the length of any portion as $2f$ to m; but as this distance is to the length, so is the depth to the radius of curvature, or $2f:m::b:\frac{bm}{2f}$, which is the radius of curvature when $a=\frac{1}{6}b$. But when a varies, the curvature will vary in the same ratio; for the curvature is proportional to the angle of the triangles representing the forces, and the angles of either triangle to its area divided by the square of its length; but the force exerted by the remoter part of the column is to f as $a+\frac{bb}{36a}-\frac{1}{3}b$ to $\frac{2}{3}b$, or as $(6a-b)^2$ to $24ab$, and is equal to $\frac{f}{24ab}\cdot(6a-b)^2$, but the square

of the side of the corresponding triangle is $\left(\frac{1}{2}b - \frac{bb}{12a}\right)^2$, or $(6a - b)^2 . \left(\frac{b}{12a}\right)^2$, consequently the force, or the area, divided by this square becomes $\frac{6af}{b^3}$, and the curvature varies directly as a, and as f, and inversely as b^3: but since m varies as b, we must make the expression for the radius of curvature $\frac{bbm}{12af}$, which becomes $\frac{bm}{2f}$ when $a = \frac{1}{6}b$, and which varies as b^3 directly, and as a and f inversely.

If the force be applied obliquely, its effect may be determined by finding the point at which it meets the perpendicular to the axis, and resolving it into two parts: that which is in the direction of this perpendicular will be counteracted by the lateral adhesion of the substance, the other will always produce the same curvature as if the force had been originally in a direction parallel to the axis: but the place of the point of indifference will be determined from the point of intersection already mentioned, and when the force becomes perpendicular to the column, the neutral point will coincide with the axis.

SCHOLIUM. If one surface of the column were incompressible, and all the resistance of its strata were collected in the other, the radius of curvature would evidently be $\frac{bbm}{af}$; a being the distance from the incompressible side, which is ultimately 12 times as great as in the natural state of an elastic substance.

5. THEOREM. The distance of the point of greatest curvature of a prismatic beam, from the line of direction of the force, is twice the versed sine of that arc of the circle of greatest curvature, of which the extremity is parallel to that of the beam.

Supposing the curve, into which the beam is bent, to be described with an equable angular velocity, its fluxion will be directly as the radius of curvature, or inversely as a, the distance of the force from the axis of the beam; this we may still call a at the point of greatest curvature, and y elsewhere, the corresponding arc of the circle of curvature being z; then the fluxion of the curve will be $\frac{a\dot{z}}{y}$; but this fluxion is to $\dot{y}$ as the radius r to the sine of the angle or arc z, or $\mp\, \dot{y} = \frac{a\dot{z}}{y} \cdot \frac{\text{sin.}\, z}{r}$, but $\frac{(\text{sin.}\, z)\dot{z}}{r} = \dot{v}$, v being the versed sine of the arc z, $\mp\, y\dot{y} = a\dot{v}$, and $yy = b \mp 2av$, b being a constant quantity: when $y = a$, $v = 0$, and $aa = b$, therefore $yy = aa - 2av$, and when $y = 0$, $aa = 2av$, and $a = 2v$.

SCHOLIUM. When the force is longitudinal, and the curvature inconsiderable, the form coincides with the harmonic curve, the curvature being proportional to the distance from the axis: and the distance of the point of indifference from the axis becomes the secant of an arc proportional to the distance from the middle of the column.

6. **THEOREM. If a beam is naturally of the form which a prismatic beam would acquire, if it were slightly bent by a longitudinal force, calling its depth, b, its length, e, the circumference of a circle of which the diameter is unity, c, the weight of the modulus of elasticity, m, the natural deviation from the rectilinear form, d, and a force applied at the extremities of the axis, f, the total deviation from the rectilinear form will be $a = \frac{bbccdm}{bbccm - 12eef}$.**

The form being originally a harmonic curve, the curvature and length of the ordinate added at each point by the action of the force will also be equal to those of a harmonic curve, of which the vertical radius of curvature must be $\frac{bbm}{12af}$, and the basis the length of the beam; but the vertical ordinate of the harmonic curve is a third proportional to its radius of curvature and that of the figure of sines on the same basis, which in this case would be $\frac{e}{c}$, the additional vertical ordinate must therefore be $\frac{ee}{cc} \cdot \frac{12af}{bbm}$, and this added to the deviation d, must become equal to a, and $a = d + \frac{ee}{cc} \cdot \frac{12af}{bbm}$, $a - \frac{ee}{cc} \cdot \frac{12af}{bbm} = d = \frac{abbccm - 12aeef}{bbccm}$, and $a = \frac{bbccdm}{bbccm - 12eef}$.

SCHOLIUM. It appears from this formula, that when the other quantities remain unaltered, a varies in proportion to d, and if $d = 0$, the beam cannot be retained in a state of inflection, while the denominator of the fraction remains a finite quantity: but when $bbccm = 12eef$, a becomes infinite, whatever may be the magnitude of d, and the force will overpower the beam, or will at least cause it to bend so much as to derange the operation of the forces concerned. In this case $f = \left(\frac{bc}{e}\right)^2 \cdot \frac{m}{12} = .8225 \frac{bb}{ee} m$, which is the force capable of holding the beam in equilibrium in any inconsiderable degree of curvature. Hence the modulus being known for any substance, we may determine at once the weight which a given bar nearly straight is capable of supporting. For instance in fir wood, supposing its height 10,000,000 feet, a bar an inch square and ten feet long may begin to bend with the weight of a bar of the same thick-

ness, equal in length to $.8225 \times \frac{1}{120 \times 120} \times 10{,}000{,}000$ feet, or 571 feet; that is with a weight of about 120 pounds; neglecting the effect of the weight of the bar itself. In the same manner the strength of a bar of any other substance may be determined, either from direct experiments on its flexure, or from the sounds that it produces. If $f = \frac{m}{n}$ $\frac{e^2}{b^2} = .8225n$, and $\frac{e}{b} = \sqrt{(.8225n)} = .907\sqrt{n}$; whence, if we know the force required to crush a bar or column, we may calculate what must be the proportion of its length to its depth, in order that it may begin to bend rather than be crushed. The height of the modulus of elasticity for iron or steel is about 9,000,000 feet, for wood, from 4,000,000 to 10,000,000, and for stone probably about 5,000,000; its weight for a square inch of iron 30,000,000 pounds, of wood from 1,500,000 to 4,000,000, and of stone about 5,000,000: and the values of n are in the two first cases from 200 to 250, and in the third about 2500, and $\sqrt{n}$ becomes 15 and 50, and $\frac{e}{b}$, 12.3 and 41.1 respectively, so that a column of iron or wood cannot support, without being crushed, a longitudinal force sufficient to bend it, unless its length be greater than 12 times its depth, nor a column of stone, unless its length be greater than 40 times its depth.

7. **Theorem.** When a longitudinal force is applied to the extremities of a straight prismatic beam, at the distance a from the axis, the deflection of the middle of the beam will be $a\,.\,(\text{sec. arc}\,(\sqrt{(\frac{3f}{m})}\,.\,\frac{e}{b}) - 1)$.

If we suppose the length to be increased until $f = \left(\frac{bc}{e}\right)^2 . \frac{m}{12}$, or $e = bc$ $\sqrt{\left(\frac{m}{12f}\right)}$, the beam might be retained by the force f in the form of a harmonic curve, of which a might be an ordinate, and the vertical ordinate would be as much greater than a as the radius is greater than the sine of the arc corresponding to its distance from the origin of the curve, or as the secant of the arc corresponding to its distance from the middle of the curve is greater than the radius, and the excess of this secant above the radius will express the deflection produced by the action of the force; but this arc is to the quadrant $\frac{c}{2}$ as e to $bc\sqrt{\left(\frac{m}{12f}\right)}$, and is therefore equal to $\sqrt{\left(\frac{3f}{m}\right)} . \frac{e}{b}$.

Scholium. Hence it appears that when the other quantities are

constant, the deflection varies in the simple ratio of a. The radius of curvature at the vertex is $\dfrac{bbm}{12af.\left(\text{sec. arc}\sqrt{\left(\frac{3f}{m}\right)}.\frac{e}{b}\right)}$, from which the degree of extension and compression of the substance may be determined.

8. **Theorem. The form of an elastic bar, fixed at one end, and bearing a weight at the extremity, becomes ultimately a cubic parabola, and the depression is $\frac{2}{3}$ of the versed sine of an equal arc, in the smallest circle of curvature.**

The ordinate of the cubic parabola being ax^3, its fluxion is $2ax^2\dot{x}$, and its second fluxion $4ax\dot{x}\dot{x}$, which varies as x the absciss. If the curvature had been constant, the second fluxion would have been $b\dot{x}\dot{x}$, the first fluxion $bx\dot{x}$, and the ordinate $\frac{1}{2}bxx$; but as it is $b\dot{x}\dot{x}-x\dot{x}\dot{x}$, the first fluxion is $bx\dot{x}-\frac{1}{2}x^2\dot{x}$, and the fluent $\frac{1}{2}bx^2-\frac{1}{6}x^3$, which, when $b=x$, becomes $\frac{1}{3}b^3$, instead of $\frac{1}{2}$.

9. **Theorem. The weight of the modulus of the elasticity of a bar is to a weight acting at its extremity only, as four times the cube of the length to the product of the square of the depth and the depression.**

If the depression be d, the versed sine of an equal arc in the smallest circle of curvature will be $\frac{3}{2}d$, and the radius of curvature $\frac{ee}{3d}$, e being the length; but the radius of curvature is also expressed by $\frac{bbm}{12af}$, a being here equal to e, therefore $\frac{ee}{3d}=\frac{bbm}{12ef}$, $12e^3f=3bbdm$, and $m=\frac{4e^3}{bbd}.f$. If f be the weight of a portion of the beam of which the length is g, the height of the modulus will be $\frac{4e^3}{bbd}.g$.

Scholium. In an experiment on a bar of iron, mentioned by Mr. Banks, e was 18 inches, b and d each 1, f 480 pounds, and g about 150 feet: hence the height of the modulus could not have been less than 3,500,000 feet. But d was probably much less than this, as the depression was only measured at the point of breaking, and m must have been larger in the same proportion.

10. **Theorem. If an equable bar be fixed horizontally at one end, and bent by its own weight, the depression at the extremity will be half the versed sine of an equal arc in the circle of curvature at the fixed point.**

The strain on each part is here equal to the weight of the portion beyond it, acting at the end of a lever of half its length: the curvature will therefore be as the square of the distance from the extremity. And if the second fluxion at the vertex be $aa\dot{x}\dot{x}$, it will be everywhere $(a-x)^2 \dot{x}\dot{x} = aa\dot{x}\dot{x} - 2ax\dot{x}\dot{x} + x^2\dot{x}\dot{x}$; the first fluxions of these quantities are $aax\dot{x}$ and $aax\dot{x} - ax^2\dot{x} + \frac{1}{3}x^3\dot{x}$, and the fluents $\frac{1}{2}a^2x^2$, and $\frac{1}{2}a^2x^2 - \frac{1}{3}ax^3 + \frac{1}{12}x^4$; or when $x=a$, $\frac{1}{2}a^4$ and $\frac{1}{4}a^4$; therefore the depression is in this case half of the versed sine.

11. Theorem. The height of the modulus of the elasticity of a bar, fixed at one end, and depressed by its own weight, is half as much more as the fourth power of the length divided by the product of the square of the depth and the depression.

The weight of the bar operates as if it were concentrated at the distance of half the length, or as if it were reduced to one half, acting at the extremity: we have therefore $\frac{e}{2}$ for the length of a portion equivalent to the weight, and $\frac{ee}{4d} = \frac{bbm}{12ef}$, whence $m = \frac{3e^3}{bbd}f$, and the height $\frac{3e^4}{2bbd}$.

12. Theorem. The depression of the middle of a bar supported at both ends, produced by its own weight, is five-sixths of the versed sine of half the equal arc in the circle of least curvature.

The curvature varies as $aa-xx$, and the second fluxion is therefore represented by $aa\dot{x}\dot{x} - xx\dot{x}\dot{x}$, while that of the versed sine is $aa\dot{x}\dot{x}$, the first fluxions are $aax\dot{x}$ and $aax\dot{x} - \frac{1}{3}x^3\dot{x}$, and the fluents $\frac{1}{2}a^2x^2$ and $\frac{1}{2}a^2x^2 - \frac{1}{12}x^4$, or, when $x=a$, $\frac{1}{2}a^4$, and $\frac{5}{12}a^4$, which are in the ratio of 6 to 5.

13. Theorem. The height of the modulus of the elasticity of a bar, supported at both ends, is $\frac{5}{32}$ of the fourth power of the length, divided by the product of the depression and the square of the depth.

For the strain at the middle is equal to the effect of the weight of one fourth of the bar acting on a lever of half the length (312); and the radius of curvature there is $\frac{5ee}{48d} = \frac{bbm}{6ef}$, and $m = \frac{5e^3f}{8bbd}$, and the height $\frac{5e^4}{32bbd}$, substituting $\frac{e}{4}$ for f.

SCHOLIUM. From an experiment made by Mr. Leslie on a bar in these circumstances, the height of the modulus of the elasticity of deal appears to be about 9,328,000 feet. Chladni's observations on the sounds of fir wood, afford very nearly the same result.

14. THEOREM. The weight under which a vertical bar not fixed at the end, may begin to bend, is to any weight laid on the middle of the same bar, when supported at the extremities in a horizontal position, nearly in the ratio of $\frac{514}{10000}$ of the length to the depression.

For the weight laid on the bar being f, the pressure on each fulcrum is $\frac{f}{2}$, and the length of the lever $\frac{a}{2}$, so that the weight of the modulus becomes $\frac{a^3 f}{4bbd}$; but the force capable of keeping the column bent is $\left(\frac{bc}{e}\right)^2 \cdot \frac{m}{12}$, or since $e = a$, $\frac{acc}{48d} \cdot f = .0514 \frac{a}{d} f$. The effect of the weight of the bar on the depression may be separately observed and deducted.

15. THEOREM. The force acting on any point of a uniform elastic rod, bent a little from the axis, varies as the second fluxion of the curvature, or as the fourth fluxion of the ordinate.

For if we consider the rod as composed of an infinite number of small inflexible pieces, united by elastic joints, the strain, produced by the elasticity of each joint, must be considered as the cause of two effects, a force tending to press the joint towards its concave side, and a force half as great as this, urging the remoter extremities of the pieces in a contrary direction; for it is only by external pressures, applied so as to counteract these three forces, that the pieces can be held in equilibrium. Now when the force, acting against the convex side of each joint, is equal to the sum of the forces derived from the flexure of the two neighbouring joints, the whole will remain in equilibrium; and this will be the case whether the curvature be equal throughout, or vary uniformly, since in either case the curvature at any point is equal to the half sum of the neighbouring curvatures; and it is only the difference of the curvature from this half sum, which is as the second fluxion of the curvature, that determines the accelerating force.

16. DEFINITION The stiffness of bodies is measured by their resistance at an equal linear deviation from their natural position

17. **Theorem.** **The stiffness of a beam is directly as its breadth, and as the cube of its depth, and inversely as the cube of its length.**

Since $m = \frac{4e^3}{bbd} . f$ (Art. 9) and m varies as bh, h being the breadth, b^3dh varies as e^3f, and f as $\frac{b^3dh}{e^3}$, that is, when d is given, as h, and b^3, and inversely as e^3.

18. **Theorem.** **The direct cohesive or repulsive strength of a body is in the joint ratio of its primitive elasticity, of its toughness, and the magnitude of its section.**

Since the force required to produce a given extension is as the extension, where the elasticity is equal, the force at the instant of breaking is as the extension which the body will bear without breaking, or as its toughness. And the force of each particle being equal, the whole force must be as the number of the particles, or as the section.

Scholium. Though most natural substances appear in their intimate constitution to be perfectly elastic, yet it often happens that their toughness with respect to extension and compression differs very materially. In general, bodies are said to have less toughness in resisting extension than compression.

19. **Theorem.** **The transverse strength of a beam is directly as the breadth and as the square of the depth, and inversely as the length.**

The strength is limited by the extension or compression which the substance will bear without failing; the curvature at the instant of fracture must therefore be inversely as the depth, and the radius of curvature as the depth, or $\frac{bbm}{12af}$ as b, consequently bm must be as af, and f as $\frac{bm}{a}$, or, since m is as bh, as $\frac{bbh}{a}$.

Scholium. If one of the surfaces of a beam were incompressible, and the cohesive force of all its strata collected in the other, its strength would be six times as great as in the natural state; for the radius of curvature would be $\frac{bbm}{af}$, which could not be less than twice as great as in the natural state, because the strata would be twice as much extended, with the same curvature, as when the neutral point is in the axis; and f would then be six times as great.

20. Definition. The resilience of a beam may be considered as proportional to the height from which a given body must fall to break it.

21. Theorem. The resilience of prismatic beams is simply as their bulk.

The space through which the force or stiffness of a beam acts, in generating or destroying motion, is determined by the curvature that it will bear without breaking; and this curvature is inversely as the depth; consequently the depression will be as the square of the length directly, and as the depth inversely: but the force in similar parts of the spaces to be described is every where as the strength, or as the square of the depth directly, and as the length inversely: therefore the joint ratio of the spaces and the forces is the ratio of the products of the length by the depth; but this ratio is that of the squares of the velocities generated or destroyed, or of the heights from which a body must fall to acquire these velocities. And if the breadth vary, the force will obviously vary in the same ratio; therefore the resilience will be in the joint ratio of the length, breadth, and depth.

22. Theorem. The stiffest beam that can be cut out of a given cylinder is that of which the depth is to the breadth as the square root of 3 to 1, and the strongest as the square root of 2 to 1; but the most resilient will be that which has its depth and breadth equal.

Let the diameter or diagonal be a, and the breadth x; then the depth being $\sqrt{(aa-xx)}$, the stiffness is $(aa-xx)^{\frac{3}{2}}x$, and the strength $aax-x^3$, which must be maximums; and $(aa-xx)^3xx$ must be a maximum; so that $3(aa-xx)^2.(-2x\dot{x}).xx+(aa-xx)^3(2x\dot{x})=o$, $aa-xx=3xx$; and the squares of the breadth and depth are as 1 to 3; also $aa\dot{x}=3x^2\dot{x}$, $x=\sqrt{\frac{1}{3}}a$, and the depth $\sqrt{\frac{2}{3}}a$, for the strongest form. It is evident that the bulk, and consequently the resilience, will be greatest when the depth and breadth are equal.

23. Theorem. Supposing a tube of evanescent thickness to be expanded into a similar tube of greater diameter, but of equal length, the quantity of matter remaining the same, the strength will be increased in the ratio of the diameter, and the stiffness in the ratio of the square of the diameter, but the resilience will remain unaltered.

For the quantity of matter remaining the same, its action is in both cases simply as its distance from the fulcrum, or from the axis of motion, and this distance is simply as the diameter, since the section remains similar in all its parts: the tension at a given angular flexure being also increased with the distance, the stiffness will be as the square of the distance, and the force in similar parts of the space described being always inversely as the space, the square of the velocity produced or destroyed will remain unaltered.

Scholium. When a beam of finite thickness is made hollow, retaining the same quantity of matter, the strength is increased in a ratio somewhat greater than that of the diameter, because the tension of the internal fibres at the instant of breaking is increased.

24. Theorem. **The stiffness of a cylinder is to that of its circumscribing prism as three times the bulk of the cylinder to four times that of the prism.**

The force of each stratum of the cylinder may be considered as acting on a lever of which the length is equal to its distance x from the axis: for although there is no fixed fulcrum at the axis, yet the whole force is exactly the same as if such a fulcrum were placed there, since the opposite actions of the opposite parts would remove all pressure from the fulcrum. The tension of each stratum being also as the distance x, and the breadth being called $2y$, the fluxion of the force on either side of the axis will be $2x^2y\dot{x}$, while that of the force of the prism is $2x^2\dot{x}$, and its fluent $\frac{2}{3}x^3$. But the fluent of $2x^2y\dot{x}$, or $2\sqrt{(1-xx)}x^2\dot{x}$, calling the radius unity, is $\frac{1}{2}(z-y^3x)$, z being the area of the portion of the section included between the stratum and the axis, of which the fluxion is $y\dot{x}$; for the fluxion of $z-y^3x$ is $y\dot{x}-y^3\dot{x}-3y^2x\dot{y}=yx^2\dot{x}-3y^2x\,.\left(\frac{-x\dot{x}}{y}\right)=yx^2\dot{x}+3yx^2\dot{x}=4yx^2\dot{x}$; and when $x=1$, and $y=0$, the fluent becomes $\frac{1}{2}z$, while the force of the prism is expressed by $\frac{2}{3}$.

Scholium. It is obvious that the strength and resilience are in this case in the same ratio as the stiffness. The strength of a tube may be found by deducting from the strength of the whole cylinder that of the part removed, reduced in the ratio of the diameters.

25. Theorem. **If a column, subjected to a longitudinal force, be cut out of a plank or slab of equable depth, in order that the extension and compression of the surfaces may be initially every where equal, its outline must be a circular arc.**

Neglecting the distance of the neutral point from the axis, the curva-

ture must be constant, in order that the tension of the superficial fibres may be equal; and the breadth must be as the distance of the line of application of the force; that is, as the ordinate of a circular arc, or, when the curvature is small, it must be equal to the ordinate of another circular arc, of which the chord is equal to the axis.

26. Theorem. **If a column be cut out of a plank of equable breadth, and the outline limiting its depth be composed of two triangles, joined at their bases, the tension of the surfaces produced by a longitudinal force will be every where equal, when the radius of curvature at the middle becomes equal to half the length of the column; and in this case the curve will be a cycloid.**

For in the cycloid, the radius of curvature varies as the distance, in the curve, from its origin, or as the square root of the ordinate a, and if the depth b be as this distance, a will vary as bb, and the curvature, which is proportional to $\frac{a}{b^3}$, will be always as $\frac{1}{b}$, and the tension will be equable throughout. In every cycloid the radius of curvature at the middle point is half of the length.

Scholium. When the curvature at the middle differs from that of the cycloid, the figure of the column becomes of more difficult investigation. It may however be delineated mechanically, making both the depth of the column and its radius of curvature proportional always to $\sqrt{a}$. If the breadth of the column vary in the same proportion as the depth, they must both be every where as the cube root of a.

No. XLVII.

SOME PROPOSITIONS ON

WAVES AND SOUND.

Partly from Dr. Young's Elementary Illustrations of the Celestial Mechanics of Laplace, and partly from the Mathematical Elements of Natural Philosophy in the second volume of his Lectures, p. 63.

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1. THEOREM. When the surface of an incompressible fluid, contained in a narrow prismatic canal, is elevated or depressed a little at any part above the general level; if we suppose a point to move in the surface each way, with a velocity equal to that of a heavy body falling through half M the depth of the fluid, the surface of the fluid, at the part first affected, will always be in a right line between the two moveable points.* Cel. Mec., No. 378, p. 318.

The particles constituting any column of the fluid, extending across the canal, are actuated by two forces, derived from the hydrostatic pressures of the columns on each side, these pressures being supposed to extend to the bottom of the canal, with an intensity regulated only by the height of the columns themselves; and this supposition would be either perfectly or very nearly true, if the particles of the fluid were infinitely elastic, that is, absolutely incompressible; and if the fluidity were at the same time so perfect, that no particle of the fluid should be affected by any pressure not tending directly towards it. A distinguished mathematician of the present day appears indeed to have assumed, that the pressure is transmitted downwards with a velocity determined by the depth, and related to the velocity of the horizontal transmission, if not identical with it: but it seems sufficiently obvious, that if the canal be supposed incompressible, the pressure must descend in it, as it confess-

* This proposition is demonstrated upon the same principles, but without the aid of symbols, in No. 395, p. 63, of the second volume of the Lectures.—*Note by the Editor.*

edly would do in an organ pipe, with a velocity dependent only on the intimate elasticity of the medium, which in this proposition is supposed infinite.*

Now the difference of the forces on each side of the thin transverse section of the canal, constituting a partial pressure, is the immediate cause of the horizontal motion; and the vertical motion is the effect of the modification of the horizontal motion: and the difference of the pressures is every where to the weight of the column or section, or of any of its parts, as the difference of the heights to the thickness of the column, or as the fluxion of the height y to that of the horizontal length of the canal x. Hence, if the weight of any particle be called g, the horizontal force acting on it will be $\frac{\mathrm{d}y}{\mathrm{d}x}g$. Such therefore is the force acting horizontally on any elementary column: but the elongation or abbreviation of the column depends on the difference of the velocities, with which its two transverse surfaces are made to advance, and this elevation or depression of the upper surface is therefore to the whole height, as the variation of the fluxion of the length, or thickness, produced by the operation of the force, is to the whole fluxion of the length; that is, δy is to y as $\delta \mathrm{d}x$ to $\mathrm{d}x$, or as $\delta \mathrm{D}x$ to $\mathrm{D}x$. But the force which produces the change being $\mathrm{d}\frac{\mathrm{d}y}{\mathrm{d}x}g = \frac{\mathrm{dd}y}{\mathrm{d}x}g$, making $\mathrm{d}x$ constant, it may be supposed to be increased, with reference to the acceleration of the upper surface of the fluid, in the ratio of the synchronous variations $\delta \mathrm{d}x$ and δy, or that of $\mathrm{d}x$ to y, and it will then become $\frac{y}{\mathrm{d}x} \cdot \frac{\mathrm{dd}y}{\mathrm{d}x}g = \frac{\mathrm{dd}y}{\mathrm{d}x^2}gy$, which will be the measure of the acceleration of the surface, and the surface will ascend or descend precisely as if immediately subjected to the operation of such a force. We may therefore inquire what must be the velocity of a body moving along the curved surface, or what must be the horizontal velocity of a similar surface moving along through the body, in order that the vertical motion should represent the effect of the force $\frac{\mathrm{dd}y}{\mathrm{d}x^2}gy$. Now in the common expression of the magnitude of a force acting in the direction of y, we say $f = \frac{\mathrm{dd}y}{\mathrm{d}t^2}$; we must therefore make $\frac{\mathrm{dd}y}{\mathrm{d}t^2} = \frac{\mathrm{dd}y}{\mathrm{d}x^2}gy$, or $\frac{\mathrm{d}x^2}{\mathrm{d}t^2} = gy$, and $\frac{\mathrm{d}x}{\mathrm{d}t} = \sqrt{(gy)}$: consequently if x flow with the constant velocity $v = \frac{\mathrm{d}x}{\mathrm{d}t} = \sqrt{(gy)}$, the second fluxion of y will always represent the actual acceleration of the surface of the fluid, the part of the curve corresponding to the time t always representing the actual position of the

* The mathematician referred to is M. Poisson.—*Note by the Editor.*

particle, as well as its motion. But $\sqrt{(gy)}$ is the velocity acquired by a body in falling through $\frac{1}{2} y$, since in general $v^2 = 2gs$, and $v = \sqrt{(2gs)}$, or $= \sqrt{(2g\text{M})}$. In this simple manner we attain a strict demonstration, on the premised supposition respecting the nature of the fluid, that the velocity of the surface will be represented by that of the surface of a wave advancing with the horizontal velocity thus determined, or, in other words, that the wave will actually advance with that velocity.

But in this form the solution is limited to the case of a wave already in progress. It may, however, readily be extended to all possible cases. For since the actions of any two or more forces are always expressed by the addition or subtraction of the results produced, in any given time, by their single operations, it may easily be understood that any two or more minute impressions may be propagated in a similar manner through the canal, without impeding each other; the inclination of the surface which is the original cause of the acting force, being the joint effect of the inclinations produced by the separate impressions, and producing singly the same force, as would have resulted from the combination of the two separate inclinations; and the elevation or depression becoming always the sum or difference of those which belong to the separate agitations. If then we suppose two similar impulses, waves, or series of waves, to meet each other in directions precisely opposite, they will still pursue their course: and at the instant when they meet in such a manner as to destroy completely each other's horizontal and vertical motions, the elevation and depression of each series will coincide and be redoubled, and the fluid will be quiescent, with an undulated surface: but in the next instant the two series will proceed uninterrupted, as before: consequently the fluid being supposed to be initially in the same state, its progressive changes will be represented by the effects of the two series of waves meeting each other, and the place of each point will be determined by the middle between the two places which it would have held by the separate effects of the two series, that is by the mean between the elevation or depression of the two points supposed in the proposition.

Corollary 1. The points, in which the similar parts of the two opposite series of waves continue to meet, will always be free from horizontal motion; hence it follows that a solid obstacle in a vertical direction might be interposed without altering the phenomenon: and consequently that any fixed obstacle meeting the waves would produce precisely the same effect on the subsequent state of either series, as is produced by the opposition of a similar series, and would reflect it in a form similar to that of the opposite series, which would have travelled

over it, if it had originated from a primitive cause of motion on the other side of the obstacle.

SCHOLIUM. It will appear, by considering the combination of the horizontal with the vertical motion, that each particle of the surface will describe an oval figure, which it will be simplest to suppose an ellipsis; the motion in the upper part of the orbit being direct with regard to the progress of the wave, and in the lower part retrograde: and the orbit will be of the same form and magnitude for each particle of the surface, when the canal is supposed to be prismatic.*

* The following Scholium is added to the demonstration of this proposition in the second volume of the Lectures:—

SCHOLIUM. The limited elasticity of liquids actually existing produces some variations in the phenomena of waves, which have not yet been investigated; but its effect may be in some degree estimated by approximation. For a finite time is actually required in order for the propagation of any effect to the parts of the fluid situated at any given depth below the surface, and for the return of the impulse or pressure to the superficial parts: so that the summit of every wave must have travelled through a certain portion of its track before the neighbouring parts of the fluid can have partaken in the whole effects which its pressure would produce by means of the displacement of the lower part of the fluid. This cause probably co-operates with the cohesion of the liquid in rounding off any sharp angles which may originally have existed; it limits the effect that an increase of depth can produce in the velocity of the transmission of waves of a finite magnitude, and diminishes the velocity of all waves the more as the depth approaches more to this limit. If the surface was originally in the form of the harmonic curve, it may be shown that the force acting at any time on a given point in consequence of the sum of the results of the forces derived from the effect of a given portion of a wave which has already passed by, will still follow the law of the same curve: but the force will be diminished in the ratio of the arc corresponding to half the space described by the wave while the impulse returns from the bottom, to its sine, the whole distance of the wave being considered as the circumference; and the velocity will be diminished in the sub-duplicate ratio; but the arc which, when diminished in the subduplicate ratio that it bears to the sine, is the greatest, is that of which the length is equal to the tangent of its excess above a right angle, or an arc of about $70^\circ\frac{1}{3}$, its sine is .94 and its length 2.8, the subduplicate ratio that of 1 to .57, and the velocity will be so much less than that which is due to the height: but with this velocity the wave will describe a portion equal to $\frac{211}{315}$ of its breadth, while the effect descends and reascends to the depth concerned; and supposing the velocity with which the impulse is transmitted through the fluid to be equal to that which is acquired by a body falling through a space equal to $\frac{1}{2}m$, and calling the depth h, and the breadth of the wave a, while $\frac{211}{315}a$ is described by v, $2h$ is described by that which is due to $\frac{1}{2}m$, or by $b\sqrt{\left(\frac{m}{2}\right)}$; and v being $.57b\sqrt{\left(\frac{h}{2}\right)}$, as $.57b\sqrt{\left(\frac{h}{2}\right)}$ to $\frac{211}{315}a$, so is $b\sqrt{\left(\frac{m}{2}\right)}$ to $2h$, and $1.14h^{\frac{3}{2}} = \frac{211}{315}a\sqrt{m}$, whence $h = .5\,(a^2m)^{\frac{1}{3}}$. For water, according to Mr. Canton's experiments, m is not more than 750,000 feet, but we may venture to call it a million; then if a, the breadth of the wave, were 1 foot, h would be 50, and the velocity nearly 23 feet in a second. If a were 1000 feet, h would be 5000; and the addition of a greater depth could not increase the velocity. Where the depth is given, the correction may be made in a similar manner. For h being in this case given, we must find the arc which is to its sine in the duplicate ratio of the velocity due to the height to the diminished velocity, represented by that arc, while that of the impulse propagated in the medium

2. **Theorem.** **The divergence of a wave makes no sensible difference in the velocity of its propagation, and its height will vary as the square root of the distance from the centre. Cel. Mec., No. 379.**

The immediate horizontal force is the same for a diverging wave as for a prismatic canal, its measure being always $\frac{\mathrm{d}y}{\mathrm{d}x}g$, as well for the parts lying without the sides of a supposed prismatic canal, as for the parts contained within it, the inclination of the surface being the same without as within those limits, and the fluxion of the height being in the same proportion to that of the length x, notwithstanding that the pressure in one direction is derived, for the extreme parts, from the surface of the collateral portion of the wave: consequently the force, as referred to the surface of the fluid, will still be expressed by $\frac{\mathrm{dd}y}{\mathrm{d}x^2}gy$. It will, however, be modified by the depression attending a progressive motion, necessary for preserving the continuity of the fluid, which must obviously be such that $-\delta y$ may be to δx, the progressive velocity, as y to x, and $\delta y = -\delta x\,\frac{y}{x}$: and the accelerative force $\frac{\mathrm{d}y}{\mathrm{d}x}g$, considered with regard to its effect at the surface, will be modified in the same proportion as the velocity, so that instead of $\frac{\mathrm{d}y}{\mathrm{d}x}g$, it will become $-\frac{\mathrm{d}y}{\mathrm{d}x}g\,\frac{y}{x} = -\frac{\mathrm{d}y}{x\mathrm{d}x}gy$, consequently the joint acceleration of the surface will be $\left(\frac{\mathrm{dd}y}{\mathrm{d}x^2} - \frac{\mathrm{d}y}{x\mathrm{d}x}\right)gy$. Now $\frac{\mathrm{dd}y}{\mathrm{d}x^2} = \frac{1}{2r}$, which is the reciprocal of the diameter of the circle of curvature, and $\frac{\mathrm{d}y}{x\mathrm{d}x}$ is the reciprocal of $x\,\frac{\mathrm{d}x}{\mathrm{d}y}$, the height of the intersection of the vertical line passing through the centre of divergence with the perpendicular to the surface of the wave, which will be very great in comparison with the diameter of curvature, when the distance from the centre becomes considerable: and the second part of the expression will become a small disturbing force, depending on the tangent of the inclination of the surface, which represents the fluent of the curvature, or of the accelerating force, and being therefore proportional to the velocity: so that like the resistance of a pendulum proportional to the velocity, it will not sensibly affect the whole period of the alternate motion, or the propagation of the wave

is expressed by twice the depth. Thus if h were 8 feet, and a 1 foot, the velocity being v, the arc must be to its sine as 256 to vv, and v to 5660 as twice the arc to twice the depth and the arc $\frac{50}{5660}$, or in degrees $.51v$; but this arc is somewhat more than 8°, and exceeds its sine so little that the velocity is scarcely diminished one thousandth by the compressibility of the water. The friction and tenacity of the water must also tend in some degree to lessen the velocity of the waves.

depending on it. We obtain the law of the diminution of the height of the waves in diverging, from the principle of the preservation of impetus, since the mass affected at once by the similar velocities increases directly as the distance from the centre x, when the depth is equable, consequently all the velocities concerned must decrease as the square root of x, in order that the sum of the masses, multiplied by the squares of the velocities, may remain constant. There will always be a continual but insensible reflection, which will preserve the centre of gravity immoveable, though it consumes no considerable part of the impetus; except at the very origin of the wave, where there seems to be something like a vibratory motion from this reflection, for a short space, at the beginning of the motion.

SCHOLIUM. It is obvious that the surface of a wave so diminishing cannot be supposed to glide on unaltered, but the demonstration shows that the motion of each point of the surface is the same as that of a surface, affected by a series of equal waves, of the magnitude of the actual wave at the given point, which is the condition supposed in the comparison of the force with the curvature.

3. THEOREM. All minute impulses are conveyed through a homogeneous elastic medium with a uniform velocity, equal to that which a heavy body would acquire, by falling through half M, the height of the medium causing the pressure. Cel. Mec., No. 380; Lectures, vol. ii., No. 400.

In this case we have to call the density y, instead of the height of an incompressible fluid in Theorem 1, p. 142, and to imagine the surface of the wave to be that of a curve representing the density by its ordinate y, which is equal to the height of a uniform column of the medium capable of producing the pressure, or in other words, to the height of the modulus of elasticity of the medium: then $\frac{dy}{dx}g$ will be the direct accelerating force, and $\frac{ddy}{dx^2}gy$ the acceleration of the ordinate of the curve of density, since here again the variation of density δy is to y, as δdx to dx: and the same conclusion is inferred, respecting the velocity with which the curve of densities must advance, in order that it may represent the instantaneous change at each point, and consequently for all the points in succession.

4. THEOREM. Every small change of form is propagated along an elastic chord, with a velocity equal to that which is due to half the length M, of a portion of the chord, of which

the weight is equal to the force producing the tension, and is reflected from the extremities in an opposite direction. Cel. Mec., No. 381 ; Lectures, vol. ii., No. 397, sch.

This proposition, though not belonging to the motions of fluids, is inserted here to complete the analogy between the height of a liquid, the modulus of elasticity of an elastic medium, and the modulus of tension of a vibrating chord. The force, impelling any small portion of the chord towards the quiescent position, or axis, is obviously expressed by the diagonal of the elementary parallelogram, formed by its extreme tangents, that is the line intercepted between the intersection of those tangents and a line equal and parallel to the second drawn from the extremity of the first, or in other words, by the second fluxion of the ordinate, when the tangent represents the first fluxion of the axis, the curve being always supposed infinitely near to the axis, and in general the force will be to the tension as the second difference $\Delta\Delta y$ to the first difference Δx: but the tension is to the weight of the element Δx as M to Δx, consequently the tension of Δx is $\frac{\mathrm{M}}{\Delta x} g$, and the accelerative force $\frac{\Delta\Delta y}{\Delta x} \cdot \frac{\mathrm{M}}{\Delta x} g = \frac{\Delta\Delta y}{\Delta x^2} \mathrm{M} g = \frac{d^2 y}{dx^2} \mathrm{M} g$, which we may make $= f = \frac{d^2 y}{dt^2}$, and we shall have $v = \sqrt{(g\mathrm{M})}$, as $v = \sqrt{(gy)}$ in Theorem 1; and the velocity will be that which is due to half the height M.

The reflection at the extremities of the chord may be represented by delineating the initial figure, and repeating it in an inverted position below the absciss: then taking, in the absciss, each way, a distance proportional to the time; and the half sum of the corresponding ordinates will indicate the place of the point at the expiration of that time.* The chord will thus represent a portion of the surface of a liquid agitated by a series of waves: and on the other hand a wave reflected backwards and forwards within a prismatic canal of its own length, abruptly terminated at each end, will exhibit a vibration precisely resembling that of an elastic chord. It may be inferred from the consideration of the motion of a chord so continued, that the point corresponding to the end of the primitive chord will always remain at rest; whence it follows that the motion of the chord, terminated by such a fixed point, must be the same as if it were continued in the manner described, the reasoning being the same as in the case of the reflection of a wave.

5. **Theorem**. When a uniform and perfectly flexible chord, extended by a given weight, is inflected into any form, differing little from a straight line, and then suffered to vibrate, it returns to its primitive state in the time which would be occupied by a

heavy body in falling through a height which is to the length of the chord as twice the weight of the chord to the tension; and the intermediate positions of each point may be found by delineating the initial figure, and repeating it in an inverted position below the absciss, then taking, in the absciss, each way, a distance proportionate to the time, and the half sum of the corresponding ordinates will indicate the place of the point at the expiration of that time. Lectures, vol. ii., No. 396.

We may first suppose the initial figure of the chord to be a harmonic curve: then the force impelling each particle will be proportional to its distance from the quiescent position, or the base of the curve. For the force acting on any element z' is to the whole force of tension p, as the element z' to the radius of curvature r, therefore the force is inversely as the radius of curvature, or directly as the curvature, that is, in this case, as the second fluxion of the ordinate; but the second fluxion of the ordinate of the harmonic curve is proportional to the ordinate itself; for the fluxion of the sine is as the cosine, and its fluxion again as the sine: the force being therefore always as the distance from a certain point, as in the cycloidal pendulum, the vibrations will be isochronous, and the ordinates will be proportionally diminished, so that the figure will be always a harmonic curve. Now calling the length of the chord a, and the greatest ordinate y, the ordinate of the figure of sines being to the length as the diameter of a circle to its circumference, or $=\frac{a}{c}$, the radius of curvature of the harmonic curve will be $\frac{aa}{ccy}$, and the force acting on the element z' will be $\frac{ccypz'}{aa}$; but the weight of the chord being q, that of z' is $\frac{qz'}{a}$, and the force is to the weight as $\frac{ccyp}{a}$ to q, or as $\frac{ccyp}{aq}$ to 1: therefore the time of vibration will be to that of a pendulum of the length y as 1 to $\surd\left(\frac{ccyp}{aq}\right)$ and to that of a pendulum of the length a in a ratio as much less as $\surd y$ is less than $\surd a$, or as 1 to $c\,.\,\surd\frac{p}{q}$. But the time of the vibration of a pendulum of the length a is to the time in which a body would fall through half a, as c to 1, consequently a single vibration of the chord will be performed in the time of falling through $\frac{a}{2}.\frac{q}{p}$, and a double vibration in the time of falling through $2a\,.\,\frac{q}{p}$. Now the element z', moving according to the law of the cycloidal pendulum, describes spaces which are

the versed sines of arcs increasing equably, and the difference of the sine at any point from the half sum of the sines of two equidifferent arcs is in a constant ratio to the versed sine of the difference, therefore, by taking the half sum of two equidistant ordinates, we find the space remaining to be described, after a time proportionate to the absciss. If the base be divided into two equal parts, and a harmonic curve be described on different sides of each part, the same demonstration is applicable to both parts, as if they were two separate chords: since the middle point will always be retained at rest by equal and opposite forces: and nothing prevents us from combining this compound vibration with the original one, since, by adding together the ordinates, we increase or diminish the fluxions and increments, in proportion to the spaces that are to be described, and the same construction of two equidistant ordinates, will determine the motion of each part. Such a compound figure may be made to pass through any two points at pleasure, and it may easily be conceived, that by subdividing the chord still further, and multiplying the subordinate curves, we may accommodate it to any greater number of points, so as to approximate infinitely near to any given figure; by which means the proposition is extended to all possible forms.

Scholium. If the initial figure consist of several equal portions crossing the axis, the chord will continue to vibrate like the same number of separate chords; and it is sometimes necessary to consider such subordinate vibrations as compounded with a general one. It usually happens also that the vibration deviates from its plane, and becomes a rotation, which is often exceedingly complicated, and may be considered as composed of various vibrations in different planes.

6. Theorem. The chord and its tension remaining the same, the time of vibration is as the length; and if the tension be changed, the frequency will be as its square root: the time also varies as the square root of the weight of the chord. Lectures, vol. ii., No. 397.

It has been shown, that the time varies in the subduplicate ratio of the force, that is, of the tension directly, and of the weight inversely; and since the weight varies as the length, the equivalent space will vary as the square of the length, and the time of describing it simply as the length.

Scholium. The properties of vibrating chords have been demonstrated in a more direct and general manner by means of a branch of the fluxionary calculus which has been called the method of variations, and which is employed in comparing the changes of the properties of a

curve existing at once in its different parts, with the variations which it undergoes in successive portions of time from an alteration of its form. An example of this mode of calculation has already been given in the investigation of the motions of waves (Theorem 1), and it may be applied with equal simplicity to the vibrations of chords, and to the propagation of sound, notwithstanding the intricacy and prolixity with which it has been always hitherto treated. It may be shown that every small change of form is propagated along an extended chord with a velocity equal to that of a heavy body falling through a height equal to half the length of a portion of the chord, of which the weight is equivalent to a force producing the tension, and which may be called the modulus of the tension; and that the change is continually reflected when it arrives at the extremities of the chord; and from this proposition all the properties of vibrating chords may be immediately deduced.

For the force, acting on any small portion of the chord, being to the tension as its length to the radius of curvature, and its weight being to the tension as its length is to the modulus of tension, the force is to the weight as the length of the modulus to the radius. By this force the whole portion is initially impelled, since the change of curvature in its immediate neighbourhood is inconsiderable with respect to the whole: and it will describe a space equal to its versed sine, which is to the arc as the arc to the diameter, in the time in which a body falling by the force of gravity would describe a space as much less, as the modulus of tension is greater than the radius, that is, a space which is to the arc as the arc to twice the modulus; and if the time be increased in the ratio of the arc to the modulus, the space described by the falling body will be increased in the duplicate ratio, and will become equal to half the modulus: If therefore a point move in the original curve with such a velocity as to describe the arc, while its versed sine is described by the motion of the chord, it would describe the length of the modulus while a heavy body would descend through half that length, and its velocity will therefore be equal to that which is acquired by a body falling through half the length: and supposing a point to move each way with such a velocity, the successive places of the given point of the chord will be initially in a straight line between these moving points. The place of the given point will also remain in a straight line between the two moving points as long as the motion continues. For the figure of the curve being initially changed in a small degree according to this law, each of the points of the chord will be found in a situation which is determined by it, and its motion will be continued in consequence of the inertia of the chord, and will receive an additional velocity from the effect of the new curvature. The space described in the first instant being equal to the mean of the versed sines of the arcs included by the

two moveable points, the velocity, as well as the second fluxion of the versed sine, may be represented by twice that mean: the increment of this velocity in the next succeeding position of the curve will be represented by the new mean of the versed sines, which is always half of the mean of the second fluxions of the ordinates on each side; for the extremities of the new elementary arcs being determined by the bisections of two equal chords removed to the distance of the arc on each side, the versed sine of each is half of the excess of the increment on one side above the increment adjoining to the corresponding one on the other side, and the sum of the versed sines is therefore half the sum of the differences of the increments from the contiguous increments on the same side, consequently the fluxion, or rather the variation of the velocity, which is represented by twice the mean versed sine, is equal to the half sum of the second fluxions of the original curve at the parts in which the moveable points are found, and the second fluxion or variation of the space, which is as the variation of the velocity, is equal to the mean of the second fluxions of the ordinates; therefore the space described is always equal to the diminution of the mean of the ordinates. And the same mode of reasoning may be extended through the whole curve. If the initial figure be such that two of its contiguous portions, lying on opposite sides of the absciss, are similar to each other, and placed in an inverted position, it is obvious that the point in which they cross the axis must remain at rest, consequently its place may be supplied by a fixed point, and either portion of the curve will continue its motion, when vibrating separately, in the same manner as if the chord were prolonged without end by a repetition of similar portions, of which the alternate ones are in an inverted position.

7. **Theorem.** When a prismatic elastic rod is fixed at one end, its vibrations are performed in the same time with those of a pendulum of which the length is $\frac{.9707 l^4}{ddh}$, l being the length, d the depth, and h the height of the modulus of elasticity: also if n denote the number of complete vibrations in a second, the measures being expressed in feet, h will be $1.1907 \frac{n^2 l^4}{d^2}$; and if a prismatic rod be loosely supported at two points only, the length of the synchronous pendulum will be $\frac{.023976 l^4}{ddh}$, and $h = .02941 \frac{n^2 l^4}{d^2}$, or $\frac{n^2 l^4}{34 d^2}$; and in this case, for a cylindrical rod of which d is the diameter, $h = \frac{2n^2 l^4}{51 d^2}$, the time of vibra-

tion being to that of the circumscribing prismatic rod as 2 to the square root of 3. Lectures, vol. ii., p. 84.

We must suppose the form of the curve, in which the rod vibrates, to be such, that all its points may perform their vibrations in a similar manner, and arrive at the line of rest at the same time; on this supposition we may determine the time in which the rod is capable of vibrating; and if the time of vibration is the same in all cases, the determination will hold good in all; if not, the problem is not capable of a general resolution; but there appears to be little or no difference in the simple sounds excited in various manners, this variety arising principally from a combination of secondary sounds. The form of the curve must therefore be such, that the fourth fluxion of the ordinate may be proportional to the ordinate itself; its equation may be found either by means of logarithmic and angular measures, or more simply by an infinite series.

The conditions of the vibration must determine the value of the co-efficients: supposing the loose extremity to be the origin of the curve, the curvature and its fluxion must begin from nothing: for the curvature at the end cannot be finite, nor can its fluxion be finite, since in these cases, an infinite force, or a finite force applied to an infinitely small portion of the rod, would be required, and the force could not be proportional to the ordinate; the initial ordinate must also be independent of the absciss; in the case of a rod fixed at the end, the ordinate and its fluxion must both vanish at the fixed point; and in the case of a rod not fixed, the second and third fluxions of the ordinate must also vanish at the remoter end, and the centre of gravity of the curve must remain in the quiescent line, the whole area, considered as belonging to either side of the basis, becoming equal to nothing; a condition which will be found identical with that of the third fluxion vanishing at the remoter end.

The series for a curve, in which the fourth fluxion of the ordinate is to be as the ordinate, can only be of this form, $y = a + \frac{bax^4}{2.3.4.l^4} + \frac{b^2ax^8}{2.3.4.5.6.7.8l^8} + \ldots + \frac{cax}{l} + \frac{bcax^5}{2.3.4.5l^5} + \frac{b^2cax^9}{2..9l^9} + \ldots + \frac{dax^2}{l^2} + \frac{bdax^6}{3..6l^6} + \ldots + \frac{cax^3}{l^3} + \frac{bcax^7}{4..7l^7} + \ldots$, for the fourth fluxion of this expression, divided by b, is of the same form with the expression itself; and the number of terms allows it to fulfil all the conditions that may be required. In both the cases here proposed, the co-efficients d and e vanish, because the second and third fluxions are initially evanescent, and the equation becomes $y = a + \frac{bax^4}{2..4l^4} + \frac{b^2ax^8}{2..8l^8} + \frac{b^3ax^{12}}{2..12l^{12}} + \ldots + \frac{cax}{l} + \frac{bcax^5}{2..5l^5} + \frac{b^2cax^9}{2..9l^9} + \frac{b^3cax^{13}}{2..13l^{13}} + \ldots$ In the first case, when $x = l$,

or $\frac{x}{l}=1$, $y=0$, and $dy=0$, whence $1+\frac{b}{2..4}+\frac{b^2}{2..8}+\frac{b^3}{2..12}+\ldots+c+\frac{bc}{2..5}+\frac{b^2c}{2..9}+\frac{b^3c}{2..13}+\ldots=0$, and $\frac{b}{2.3}+\frac{b^2}{2..7}+\frac{b^3}{2..11}+\ldots+c+\frac{bc}{2..4}+\frac{b^2c}{2..8}+\frac{b^3c}{2..12}+\ldots=0$; therefore $-c=$

$\dfrac{1+\frac{b}{2.4}+\frac{b^2}{2..8}+\frac{b^3}{2..12}+\ldots}{1+\frac{b}{2..5}+\frac{b^2}{2..9}+\frac{b^3}{2..13}+\ldots}$, and $=$

$\dfrac{\frac{b}{2.3}+\frac{b^2}{2..7}+\frac{b^3}{2..11}+\ldots}{1+\frac{b}{2.4}+\frac{b^2}{2..8}+\frac{b^3}{2..12}+\ldots}$. Hence, by multiplying the numerator of each fraction by the denominator of the other, and arranging the products according to the powers of b, we obtain the equation $1-\frac{1}{3.4}b+\frac{4}{3..8}b^2-\frac{16}{3..12}b^3+\ldots=0$, which has an infinite number of roots; the first two being $b=12.3623$, and $b=489.4$. In a similar manner we obtain, for the second case, making the second fluxion of y, and either its third fluxion, or the area, vanish when $x=l$, the equation $\frac{1}{3.4}-\frac{4}{3..8}b+\frac{16}{3..12}b^2-\frac{64}{3..16}b^3+\ldots=0$; and of this the first two roots are $b=500.5$ and $b=3803$. From these values of b, those of c may be readily found; and for each value after the first, the rod has an additional quiescent point.

In order to determine the time of vibration, we must compare the force acting on a particle x' at the end of the rod with its weight. The force is $\frac{bbm}{12ar}$ (supra, p. 130, Theorem 4), a being equal to $\frac{1}{2}x$, r to $\frac{dx^2}{d^2y}$ (the chord of the circle of curvature), and b being the depth, which we may here call d: but the weight of the particle x' is $\frac{m}{h}x'$, and the force is to that of gravity as $\frac{ddh}{6x'x'}\cdot\frac{d^2y}{dx^2}$ is to unity. Now $\frac{d^2y}{dx^2}=\frac{bax'^2}{2l^4}$; for, when x is evanescent, the subsequent terms are inconsiderable in comparison with this, and the force is $\frac{baddh}{12l^4}$, the space to be described being a; and if the space became $\frac{12l^4}{bddh}$, and the force equal to that of gravity, the vibration would be performed in the same time; this is therefore the length of the synchronous pendulum; that is, for the fundamental sound, in the first case $\frac{.9707l^4}{ddh}$, and in the second $.023976\frac{l^4}{ddh}$.

A pendulum, of which the length is $\frac{.9707l^4}{ddh}$ feet, makes $\frac{d}{l^2}\sqrt{\left(\frac{h}{.9707}\cdot\frac{39.13}{12}\right)}$ vibrations in a second, and $\frac{d}{2l^2}\sqrt{\left(\frac{h}{.9707}\cdot\frac{39.13}{12}\right)}=n$ double vibrations, such as are considered in the estimation of musical sounds. Hence $h=1.1907\left(\frac{nll}{d}\right)^2$. And in the same manner, for a rod loosely supported at two points, $h=.02941\left(\frac{nll}{d}\right)^2$.

When the rod is loosely fixed at both ends, the figure coincides with the harmonic curve, and the length of the equivalent pendulum is $\frac{12l^4}{c^4d^2h}$, c being 3.1416, and c^4, or b, 97.41.

If a prismatic bar supported at the extremities, be depressed by a weight equal to a portion of itself of which the length is gl, the depression being e, h will be $\frac{gl^4}{4dde}$, and when $h=\frac{n^2l^4}{34d^2}$, $n^2=8.5\frac{g}{e}$, e being expressed in feet. The weight under which the bar may begin to bend (supra, p. 132, Theorem 6) will be equal to that of a portion of which the length is $.0242n^2l^2$.

The stiffness of a cylinder being to that of its circumscribing prism as three times its mass to four times that of the prism, the relative force will be $\frac{3}{4}$ as great as in the prism, and the time will be increased in the subduplicate ratio, or as 1 to .866. If a cylinder be compared with a prism of the same length and weight, its vibrations will be less frequent in the ratio of 300 to 307, or nearly of 43 to 44.

The second values of b show the proportion of the first harmonic or secondary sounds of the rods, the length of the synchronous pendulum being diminished in the ratios of 1 to 39.59, and 1 to 7.6, and the times of vibration in the ratios of 1 to 6.292, and of 1 to 2.757.

Scholium. All these results are amply confirmed by experiment, and they afford an easy method of comparing the elasticity of various substances. In a tuning fork of steel, l was 2.8 inches, d .125, and $n=512$, hence h is about 8 530 000 feet. In a plate of brass, held loosely about one fifth of its length from one end, l was 6.2 inches, $d=.072$, and $n=273$, whence $h=4\ 940\ 000$; in a wire of inferior brass, l being 20 inches, d .225, and $n=74$, h appears to be 4 700 000. A plate of crown glass, 6.2 inches long and .05 thick, produced a sound consisting of 284 vibrations in a second, whence $h=9\ 610\ 000$ feet. A box scale .012 f. thick, and 1.01 f. long, gave 154 vibrations, hence $h=5\ 050\ 000$ feet. When these substances were held in the middle, the note became higher by an octave and somewhat more than a fourth. Riccati found the difference between the elasticities of steel and brass somewhat greater than this. For ice, h appeared to be about 850 000.

Two small rods of deal, one foot in length, produced sounds, consisting of 270 and 384 vibrations in a second; their weights were 153 and 127 grains respectively: hence the formula $.0242n^2l^2$ gives nearly 35 and 65 pounds for the force under which they would bend; the experiment, which was made somewhat hastily, gave 36 and 50.

8. **Definition.** **A sound, of which the number of vibrations in a second is any integer power of 2, is denoted in music by the letter c. Lectures, vol. ii., p. 67, No. 399.**

Scholium. Hence we may form a table of the number of vibrations of each note in a second.

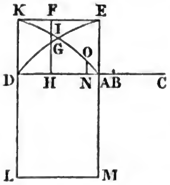
$\left(\begin{smallmatrix}C\\6\end{smallmatrix}\right)$	$\underline{\underline{C}}$	$\underline{C}$	C	c	$\bar{c}$	$\bar{\bar{c}}$	$\bar{\bar{\bar{c}}}$	$\overset{4}{c}$	$\overset{8}{c}$
1	16	32	64	128	256	512	1024	2048	32768

	$\bar{c}$	$\bar{c}\sharp$	$\bar{d}$	$\bar{e}\flat$	$\bar{e}$	$\bar{f}$	$\bar{f}\sharp$	$\bar{g}$	$\bar{a}\flat$	$\bar{a}$	$\bar{b}\flat$	$\bar{b}$	$\bar{\bar{c}}$
Scales of C.	256		288	307	320	341		384	409	427	451	480	512.
Equal temperament	256	271	287	304	323	342	362	384	406	431	456	483	512.
Progressive temperaments	256	270	287	303	321	341	360	383	405	427	455	481	512.

9. **Theorem.** **All minute impulses are conveyed through a homogeneous elastic medium with a uniform velocity, equal to that which a heavy body would acquire by falling through half the height of the medium causing the pressure. Lectures, vol. ii., No. 400, p. 68.**

If a moveable point be urged through a small space by the difference of two forces, varying inversely as its distance from two equidistant fixed points, in the same right line, the times of describing that space will be ultimately equal, whatever be its magnitude. For, calling the distance of each point a, and the space to be described x, the forces will be $\frac{1}{a-x}$ and $\frac{1}{a+x}$, and their difference $\frac{2x}{aa-xx}$, which is to $\frac{1}{a}$ as $2x$ to $a-\frac{xx}{a}$; but since x is evanescent, this ratio becomes that of $2x$ to a, and the force varies as the space to be described, consequently the times

are equal. If therefore all the particles of an elastic medium contiguous to any plane, be agitated at the same time by a motion varying according to any law, they will communicate a motion to the particles on each side, and this motion will be propagated in each direction with a uniform velocity, and so that each particle shall observe the same law in its motion. For as in the collisions of elastic balls, each ball communicates its whole motion to the next, and then remains at rest, so each particle of the medium will communicate its motion to the next in order; the common centre of inertia of two neighbouring particles supplying the place of a fixed point; and the retrograde motions will also be similarly communicated by the expansive force and pressure of the medium; and since the magnitude of the motion, while it is considered as evanescent, does not affect the time of its communication from one particle to the next, the velocity will not be affected by this magnitude, and the whole successive motions will be transferred to the neighbouring particles in their original order and proportion. For computing the velocity, it is convenient to assume a certain law for the motion of each particle, and it is simplest to suppose it moving according to the law of the cycloidal pendulum. Let AB be the minute space described by the particle A, in one semivibration, while the undulation is transmitted through AC = DA, and let DE be a figure of sines, of which DA is the half basis; then if EF flow uniformly with the time, that is, if it increase with the velocity of the undulation, the versed sine FG will be in a constant ratio to the motion of A; the velocity of A will be as the fluxion of the space, or of FG, that is, as the conjugate ordinate HI; the force will be as the fluxion of the velocity, or as FG; and the force being as the change of density, or as its fluxion, the density, or rather the excess above the natural density, will be again as HI, and the fluent of the product of HI into the fluxion of the base, will give the whole excess of density in DA, which will therefore be represented by the figure DAK. But when A arrives at B, the beginning of the undulation reaches C, and the whole fluid which occupied A is condensed into BC, so that its mean density is increased in the ratio of AC to BC, and AB represents the excess above the natural density; therefore let the rectangle DLMA be to DAK, or DKq, as BC to AB, or ultimately as AC or DA to AB; that is, let DA.DL : DKq :: DA : AB, or $DL = \frac{DKq}{AB}$, then DL will represent the natural density, while the ordinates HI everywhere represent its increase. Let NA be the evanescent length of the particle A, then the force actuating



it will be as the difference, of the densities at its extremities, or as NO, which is equal to NA; therefore the force impelling A, is to the whole elasticity, as NA to DL. Now if h be the height of a column of the fluid, equal in weight to the whole elasticity, this weight will be to the weight of A as h to NA; and the force impelling A being h NA : DL, this force will be to the weight of A as h to DL, or as $h \cdot \frac{AB}{DKq}$ to 1. Let there be two pendulums, of which the lengths are h and AB, then with the same force, they will vibrate in times which are as $\surd h$ and $\surd$ AB, and if the force in AB become $h \cdot \frac{AB}{DKq}$, the time being inversely in the sub-duplicate ratio of the force, the vibrations will be as $\surd h$ to $\surd AB \cdot \surd \left(\frac{DKq}{h.AB}\right)$ or as h to DK; and in the time of this semivibration in AB, the undulation will be transmitted through DA, therefore in a semivibration of h, it will be transmitted through a space greater in the ratio of h to DK, which will be to h as DA to DK, or as half the circumference of a circle to its diameter; and while a heavy body falls through half h, the undulation will describe h, its velocity will therefore be equal to the final velocity of the body falling through half h. According to this theorem the mean velocity of sound should be 946 feet in a second, h being 27880 feet, but it is found to be nearly 1130, which is one-fifth greater than the computed velocity. The most probable reason that has been assigned for this difference is the partial increase of elasticity occasioned by the heat and cold produced by condensation and expansion.

10. THEOREM. **The height of the barometer will not affect the velocity of sound; but, if the density vary, the pressure remaining the same, the velocity will vary in its subduplicate ratio. Lectures, vol. ii., No. 401, p. 69.**

For the velocity varies in the subduplicate ratio of the height of a homogeneous atmosphere, and that height remains the same while the density is only varied by means of pressure.

SCHOLIUM. The velocity of the transmission of an impulse through an elastic medium of any kind may be more generally determined without the consideration of any particular law for the variation of the density; and it may be directly demonstrated, that the velocity with which any impulse is transmitted by an elastic substance, is equal to that which is acquired by a heavy body in falling through half the height of the modulus of its elasticity. The density of the different parts of the medium, throughout the finite space, which is affected by the impulse at any one time, may be represented by the ordinates of a curve; that which corresponds to the natural density being equal to the height of

the modulus of the elasticity. The force acting on any small portion will be expressed by the difference of the ordinates at its extremities, that is, by the weight of a portion of the modulus equal in height to that difference; this force is to the weight, which is to be moved, as the fluxion of the ordinate to that of the absciss; and the velocity with which the density increases will be as the difference of the forces at the extremities of the portions, or as the second fluxion of the ordinate of the curve; and the increment of the ordinate expressing the density will be to the whole, as half of its second fluxion to its first fluxion; while therefore the density varies so as to be represented by the mean of two ordinates at a small distance on each side of the first ordinate, the increment of the ordinate being represented by the mean versed sine of the arcs, or half the second fluxion of the mean ordinate, the decrement of the space occupied by the particles will be as much less as the fluxion of the absciss is less than the ordinate, and the whole velocity being as much greater than the difference of the velocities, as the force is greater than its fluxion, or as the first fluxion of the ordinate is greater than its second fluxion, it follows that, in the same time, the particles will actually describe a space equal to half of the first fluxion of the ordinate, diminished in the ratio of the fluxion of the absciss to the ordinate; but if the force were altered in the ratio of the fluxion of the ordinate to that of the absciss, so as to become equal to that of gravity, the space described would become equal to half the fluxion of the absciss, diminished in the ratio of the fluxion of the absciss to the ordinate; and if the time were increased in the ratio of the fluxion of the absciss to the ordinate, the space described would be increased in the duplicate ratio, and would become equal to half the ordinate; and if a point move each way through the curve so as to describe an arc while the variation of density causes the ordinate to be diminished by a space equal to the mean versed sine, it would describe a space equal to the ordinate or the height of the modulus, while half that space would be described by the action of gravity; consequently the velocity of the points would be initially equal to that of a heavy body falling through half the height of the modulus. And that it would always remain equal to this velocity, so that the density of the medium might always be expressed by the mean ordinate, may be shown exactly in the same manner as has already been done with respect to the motions of waves and of vibrating chords. The variation of the velocity and the change of place of the particles may be easily deduced from the successive forms of the curve representing the density; and the whole effect may also be considered as arising from the progressive motion of the same curves which express the cotemporary affections of the different parts of the medium, and which will also show the successive states of any one portion of it at different times.

No. XLVIII.

AN INVESTIGATION OF THE PRESSURE SUSTAINED BY THE FIXED SUPPORTS OF

FLEXIBLE SUBSTANCES.

From the Philosophical Magazine for 1815, vol. xlvi. p. 139.

To Mr. Tilloch.

Sir,

The formidable accident, which occurred some time since, from the failure of the hoops of a vat of great size, has led to an inquiry respecting the strength required in structures of this kind; and its results are comprehended in the following propositions. It must be remembered that they are only correctly true upon the supposition that the resisting points are absolutely fixed, and that in actual practice the forces will be somewhat more equally divided: it would, however, be always prudent to make the strength great enough for the most unfavourable supposition that can be made respecting its employment.

A. If a flexible bar, equably loaded throughout its length, be supported at each end and in the middle by fulcrums perfectly fixed, the middle point will sustain $\frac{5}{8}$ of the whole pressure.

Let the half length be a, the distance of any point from the middle x, and the pressure on the end y; then the strain at the point, being the joint result of all the forces acting on either side of it, as on the arm of a lever of which it is the fulcrum, will be $y\ (a-x) - (a-x)\ \frac{1}{2}\ (a-x)$, since the weight of the portion $a-x$ acts at the distance $\frac{1}{2}(a-x)$; and the curvature will be as $ay - xy - \frac{1}{2}a^2 + ax - \frac{1}{2}x^2$, the

curve being supposed to differ but little from a straight line: hence the fluxion of the inclination will be as $ay\dot{x} - yx\dot{x} - \frac{1}{2} a^2\dot{x} + ax\dot{x} - \frac{1}{2} x^2\dot{x}$, the fluent $ayx - \frac{1}{2} yx^2 - \frac{1}{2} a^2x + \frac{1}{2} ax^2 - \frac{1}{6} x^3$, which requires no correction: and in the same manner the fluent of the ordinate will be found $\frac{1}{2} ayx^2 - \frac{1}{6} yx^3 - \frac{1}{4} a^2x^2 + \frac{1}{6} ax^3 - \frac{1}{24} x^4$, which must vanish when $x = a$, since the ends are supposed to be absolutely fixed, or $0 = \frac{1}{2} a^3y - \frac{1}{6} a^3y - \frac{1}{4} a^4 + \frac{1}{6} a^4 - \frac{1}{24} a^4 = \frac{1}{3} y - \frac{1}{8} a$, and $y = \frac{3}{8} a$, which is $\frac{3}{16}$ of $2a$, the whole pressure; so that the two ends support $\frac{3}{8}$ of the whole pressure, and leave $\frac{5}{8}$ for the middle.

B. In order that a flexible bar, equably loaded, may rest equally on each of three fixed supports, their distance must be ·3472 of the whole length.

If the half length be a, and the distance m, the strain, between the supports, will be $\frac{2}{3} a (m - x) - \frac{1}{2} (a - x)^2$; the inclination, by taking the fluent, is found $\frac{2}{3} amx - \frac{1}{3} ax^2 - \frac{1}{2} a^2x + \frac{1}{2} ax^2 - \frac{1}{6} x^3$, and the ordinate $\frac{1}{3} amx^2 - \frac{1}{9} ax^3 - \frac{1}{4} a^2x^2 + \frac{1}{6} ax^3 - \frac{1}{24} x^4$, which must vanish when $x = m$, and $\frac{1}{3} am - \frac{1}{9} am - \frac{1}{4} a^2 + \frac{1}{6} am - \frac{1}{24} m^2$, or $\frac{7}{18} am - \frac{1}{4} a^2 - \frac{1}{24} m^2$ must vanish also; whence $m^2 - \frac{28}{3} am = - 6a^2$, $m - \frac{14}{3} a = \pm \sqrt{\frac{142}{9}} a$, and $m = \frac{14 - \sqrt{142}}{3} a = ·6944a$; that is, about $\frac{1}{72}$ of the whole length more than if the bar were composed of three pieces, and each point supported an equal share.

C. If a flexible bar, equably loaded, rest on five fixed points, at the distance of $\frac{1}{5}$ of the length from each other, and $\frac{1}{10}$ from the ends, the pressures will be as 59, 52, and 58, or as

2107, 1857, 2071, 1857, and 2107; and if the middle support be removed, the pressure on the remaining points will be as 11 and 21, or as 1719, 3281, 3281, 1719.

Calling the whole length $10a$, and the pressure on the lateral fulcrums, y and z, the strain at the distance x from the middle will be, for the portion next the middle, $y\,(2a-x) + z(4a-x) - \frac{1}{2}\,(5a-x)^2$; the inclination $2ayx - \frac{1}{2}\,yx^2 + 4azx - \frac{1}{2}\,zx^2 - \frac{25}{2}\,a^2x + \frac{5}{2}\,ax^2 - \frac{1}{6}\,x^3$; and the ordinate $ayx^2 - \frac{1}{6}\,yx^3 + 2azx^2 - \frac{1}{6}\,zx^3 - \frac{25}{4}\,a^2x^2 + \frac{5}{6}\,ax^3 - \frac{1}{24}\,x^4$, which must vanish when $x = 2a$, and $0 = 4a^3y - \frac{4}{3}\,a^3y + 8a^3z - \frac{4}{3}\,a^3z - 25a^4 + \frac{20}{3}\,a^4 - \frac{2}{3}\,a^4 = a^3\left\{\frac{8}{3}\,y + \frac{20}{3}\,z - \frac{57}{3}\,a\right\}$, and $8y + 20z = 57a$. In the next portion the pressure y is not concerned, and the expression for the inclination becomes $4azx - \frac{1}{2}\,zx^2 - \frac{25}{2}\,a^2x + \frac{5}{2}\,ax^2 - \frac{1}{6}\,x^3 + b$; and for the ordinate $2azx^2 - \frac{1}{6}\,zx^3 - \frac{25}{4}\,a^2x^2 + \frac{5}{6}\,ax^3 - \frac{1}{24}\,x^4 + bx + c$: here b must be determined from the first portion, the final inclination of the one coinciding with the initial inclination of the other; and making in both expressions $x = 2a$, $8a^2z - 2a^2z - 25a^3 + 10a^3 - \frac{4}{3}\,a^3 + b = 4a^2y - 2a^2y + 8a^2z - 2a^2z - 25a^3 + 10a^3 - \frac{4}{3}\,a^3$; consequently $b = 2a^2y$: then the ordinate vanishing at the beginning of the second portion, when $x = 2a$, we have $0 = 8a^3z - \frac{4}{3}\,a^3z - 25a^4 + \frac{20}{3}\,a^4 - \frac{2}{3}\,a^4 + 4a^3y + c$; and when $x = 4a$, and the ordinate once more vanishes, $0 = 32a^3z - \frac{32}{3}\,a^3z - 100a^4 + \frac{160}{3}\,a^4 - \frac{32}{3}\,a^4 + 8a^3y + c$; and by subtraction, $24a^3z - \frac{28}{3}\,a^3z - 75a^4 + \frac{140}{3}\,a^4 - \frac{30}{3}\,a^4 + 4a^3y = 0 = a^3\left\{\frac{44}{3}\,z - \frac{115}{3}\,a + 4y\right\}$; whence, suppressing a^3 and subtracting $4y + 10z - \frac{57}{2}\,a$, we have $0 = \frac{14}{3}\,z - \frac{59}{6}\,a$, and $z = \frac{59}{28}\,a$: consequently $y = \frac{52}{28}\,a$, and for the pressure supported at the middle, there remains $\frac{58}{28}\,a$.

If we now suppose the fulcrum at the middle to be removed, the equations for the second part of the figure and for the inclination of the first part remain unaltered, and we have also $y + z = 5a$, and $4y + 4z = 20a$, which, subtracted from $\frac{44}{3} z - \frac{115}{3} a + 4y = 0$, leaves $\frac{32}{3} z - \frac{55}{3} a = 0$, and $z = \frac{55}{32} a$, and $y = \frac{105}{32} a$.

D. In a flexible stave, forming part of the side of a cistern, and supported only at the ends, the inclination at the top is $\frac{7}{8}$ as great as at the bottom.

The centre of pressure being at one-third of the height, the upper support must withstand $\frac{1}{3}$, and the lower $\frac{2}{3}$ of the whole force, which, if a be the height, may be called $\frac{1}{2} a^2$; and the strain at the distance x from the surface will be the difference of the strains produced by the pressure of the fluid and the resistance of the support, that is $\frac{1}{6} a^2 x - \frac{1}{6} x^3$, since the pressure of the fluid above the given point, that is $\frac{1}{2} x^2$, may be considered as united in the centre of pressure, and therefore acting at the distance $\frac{1}{3} x$. Hence, for the fluxion of the inclination of the stave, we have $\frac{1}{6} a^2 x\dot{x} - \frac{1}{6} x^3\dot{x}$, and the corrected fluent is $\frac{1}{12} a^2 x^2 - \frac{1}{24} x^4 + b$: again, for the ordinate of the curve we find, by a second integration, $\frac{1}{36} a^2 x^3 - \frac{1}{120} x^5 + bx$, which must vanish when $x = a$, so that $\frac{7}{360} a^4 + b = 0$, and $b = -\frac{7}{360} a^4$: hence, when $x = a$, the inclination becomes $\frac{1}{24} a^4 - \frac{7}{360} a^4 = \frac{8}{360} a^4$, while the initial inclination is represented by $b = -\frac{7}{360} a^4$.

E. If a stave be supported by three fixed fulcrums or hoops, one at each end, the other in the middle, the upper one will

sustain $\frac{1}{48}$ of the whole pressure, the middle $\frac{30}{48}$, and the lowermost $\frac{17}{48}$.

If we call the distance from the surface x, the pressure at the top y, and at the middle z, the strain will be first $yx - \frac{1}{6}x^3$; and below the middle, calling half the height a, $yx + z(x-a) - \frac{1}{6}x^3$, whence the inclination will be first $\frac{1}{2}yx^2 - \frac{1}{24}x^4 + b$; and secondly, $\frac{1}{2}yx^2 - \frac{1}{24}x^4 + \frac{1}{2}zx^2 - azx + c$, and the ordinate first $\frac{1}{6}yx^3 - \frac{1}{120}x^5 + bx$, and secondly $\frac{1}{6}yx^3 - \frac{1}{120}x^5 + \frac{1}{6}zx^3 - \frac{1}{2}azx^2 + cx + d$. Now the ordinate must vanish in the first expression when $x = a$: hence $\frac{1}{6}ya^2 - \frac{1}{120}a^4 + b = 0$, and the inclination in the middle is $\frac{1}{2}ya^2 - \frac{1}{24}a^4 + \frac{1}{120}a^4 - \frac{1}{6}ya^2 = \frac{1}{3}a^2y - \frac{1}{30}a^4$, which must be the value of the inclination in the second expression when $x = a$; so that $\frac{1}{2}a^2y - \frac{1}{24}a^4 + \frac{1}{2}a^2z - a^2z + c = \frac{1}{3}a^2y - \frac{1}{30}a^4$, and $c = -\frac{1}{6}a^2y + \frac{1}{120}a^4 + \frac{1}{2}a^2z$: when, therefore, $x = a$ in the second expression for the ordinate, $\frac{1}{6}a^3y - \frac{1}{120}a^5 + \frac{1}{6}a^3z - \frac{1}{2}a^3z - \frac{1}{6}a^3y + \frac{1}{120}a^5 + \frac{1}{2}a^3z + d = 0 = \frac{1}{6}a^3z + d$, and $d = -\frac{1}{6}a^3z$: when also $x = 2a$, $\frac{4}{3}a^3y - \frac{4}{15}a^5 + \frac{4}{3}a^3z - 2a^3z - \frac{1}{3}a^3y + \frac{1}{60}a^5 + a^3z - \frac{1}{6}a^3z = 0 = a^3y - \frac{1}{4}a^5 + \frac{1}{6}a^3z$, and $z = \frac{3}{2}a^2 - 6y$. Again, the pressure on the third point will be $2a^2 - y - z$, the three hoops having to sustain the pressure $2a^2$: this third pressure must also exceed the force y by $\frac{2}{3}a^2$, in order that it may hold in equilibrium the whole pressure $2a^2$, acting at the distance $\frac{1}{3}a$ from the middle point, considered as the fulcrum of a lever, so that $y + \frac{2}{3}a^2 = 2a^2 -$

$y - z$, and $z = \frac{4}{3} a^2 - 2y$; whence, subtracting the former value of z, we have $4y - \frac{1}{6} a^2 = 0$, $y = \frac{1}{24} a^2$, $z = \frac{5}{4} a^2$, and the third pressure $\frac{17}{24} a^2$.

F. If a stave be supported at the ends, and by two intermediate hoops at equal distances, the respective pressures will be $\frac{1}{45}$, $\frac{9}{45}$, $\frac{24}{45}$, and $\frac{11}{45}$.

For the first and second portions of the staves, the values of the inclinations and ordinates are determined from those of y and z, as in the last proposition: for the third, the inclination will be $\frac{1}{2} yx^2 - \frac{1}{24} x^4 + \frac{1}{2} zx^2 - azx + \frac{1}{2} ux^2 - 2aux + e$, which, at the origin of this portion, where $x = 2a$, becomes $2a^2y - \frac{2}{3} a^4 + 2a^2z - 2a^2z + 2a^2u - 4a^2u + e$, and this must be equal to the final inclination in the second part, or to $2a^2y - \frac{2}{3}a^4 + 2a^2z - 2a^2z - \frac{1}{6} a^2y + \frac{1}{120} a^4 + \frac{1}{2} a^2z$, whence $-2a^2u + e = -\frac{1}{6} a^2y + \frac{1}{120} a^4 + \frac{1}{2} a^2z$, so that the ordinate will be $\frac{1}{6} yx^3 - \frac{1}{120} x^5 + \frac{1}{6} zx^3 - \frac{1}{2} azx^2 + \frac{1}{6} ux^3 - aux^2 + 2a^2ux - \frac{1}{6} a^2yx + \frac{1}{120} a^4x + \frac{1}{2} a^2zx + f$; and this must vanish when $x = 2a$ and $x = 3a$, or $0 = \frac{4}{3} a^3y - \frac{4}{15} a^5 + \frac{4}{3} a^3z - 2a^3z + \frac{4}{3} a^3u - 4a^3u + 4a^3u - \frac{1}{3} a^3y + \frac{1}{60} a^5 + a^3z + f = \frac{9}{2} a^3y - \frac{81}{40} a^5 + \frac{9}{2} a^3z - \frac{9}{2} a^3z + \frac{9}{2} a^3u - 9a^3u + 6a^3u - \frac{1}{2} a^3y + \frac{1}{40} a^5 + \frac{3}{2} a^3z + f$, and by subtraction $3y - \frac{7}{4} a^2 + \frac{7}{6} z + \frac{1}{6} u = 0$, or $36y - 21a^2 + 14z + 2u = 0$. We have also, as before, from a comparison of the evanescent ordinates of the second portion, $z = \frac{3}{2} a^2 - 6y$, or $6z = 9a^2 - 36y$, and by addition, $-12a^2 + 8z + 2u = 0$. On the other hand, considering the stave as a lever with its fulcrum at its lower end, we have $\frac{9}{2} a^3 = 3ay + 2az + au$, and $2u = 9a^2 - 6y - 4z =$

$12a^2 - 8z$, and $6y = 4z - 3a^2 = \frac{3}{2}a^2 - z$, whence $5z = \frac{9}{2}a^2$, $z = \frac{9}{10}a^2$, and the second hoop sustains one-fifth of the pressure; consequently $y = \frac{1}{4}a^2 - \frac{1}{6}z = \frac{1}{10}a^2$, $u = \frac{24}{10}a^2$, and there remains for the force at the bottom, $\frac{11}{10}a^2$. In a similar manner the calculation may be extended step by step to a greater number of points; but as the number increases, the inequality of the distribution between the neighbouring points must of course diminish, and if it became infinite, the pressure on each would be simply as the depth.

A. B. C. D.

August 3, 1815.

No. XLIX.

AN ESSAY ON THE PRESSURE OF

SEMIFLUID AND COHESIVE SUBSTANCES.

From Hutton's Mathematical Dictionary, article 'Pressure.'*

The resistance opposed by friction, or adhesion, to the relative motion of any two given solid or semifluid substances, is nearly proportional to the force urging the surfaces into contact. Since, however, this force must necessarily be augmented by the force of direct cohesion, which is proportional to the extent of the surfaces in contact, it follows, that a portion of the resistance to lateral motion, must also, in cohesive substances, be proportional to the magnitude of the surfaces concerned, and independent of the direct pressure. The proportion of the variable resistance, to the force on which it depends, is that of the height to the horizontal extent of an inclined plane, on which the surfaces would begin to slide on each other, if this resistance only were concerned, or if the force or weight were very great, and the extent of the surface very small: and the angle formed by such a plane, with the horizon, is called the angle of repose of the substance. The mutual cohesion of two substances may be estimated from the thickness of a coat of one of the substances, which would be supported by it in contact with a vertical surface of the other; and both these properties may be practically determined, with respect to any internal surfaces or sections of a given substance, by raising a portion of it, terminated by a horizontal and a vertical surface, until the angle breaks off, observing both the depth and the breadth of the portion thus separating.

A. It is first required to determine the angle of fracture for

* Dr. Hutton, in some prefatory remarks upon this article, refers to his Course of Mathematics, vol. ii. p. 196, and vol. iii. p. 256, for a popular theory of the pressure of pretty compact or firm earth.—*Note by the Editor.*

a semifluid and cohesive substance, terminated by a horizontal and a vertical surface, and supported only by a horizontal force.

We have here a wedge of the given substance, tending to slide down an inclined plane, and to overcome at once the horizontal pressure, and the resistances in the direction of the plane derived from the cohesion, and from the friction produced by the sum of the other forces; and we are to determine the breadth x of that wedge, in which this tendency will be the greatest, its depth being a.

Now the weight of the wedge being expressed by $\frac{1}{2}ax$, its immediate tendency to descend along the inclined plane will be $\frac{1}{2}ax \cdot \frac{a}{\sqrt{(aa+xx)}}$, which will be opposed by the horizontal force f, acting in a contrary direction, and reduced to $f \frac{x}{\sqrt{(aa+xx)}}$, and by the resistance derived from three sources: the first from the cohesion, which is expressed by $b\sqrt{(aa+xx)}$, b being the thickness supported by the lateral adhesion of a vertical surface; the second and third from the two pressures, represented by $\frac{1}{2}tax \cdot \frac{x}{\sqrt{(aa+xx)}}$ and $tf \cdot \frac{a}{\sqrt{(aa+xx)}}$, where t is the tangent of the angle of repose, the resistance being to the direct or perpendicular pressure as t to 1. Hence, for the state of equilibrium, we have the equation $\frac{1}{2}ax \cdot \frac{a}{\sqrt{(aa+xx)}} = f \cdot \frac{x}{\sqrt{(aa+xx)}} + b\sqrt{(aa+xx)} + \frac{1}{2}tax \cdot \frac{x}{\sqrt{(aa+xx)}} + tf \cdot \frac{a}{\sqrt{(aa+xx)}}$, and $\frac{1}{2}a^2x = fx + b(a^2+x^2) + \frac{1}{2}atx^2 + atf$; whence $f = \frac{\frac{1}{2}aax - aab - bxx - \frac{1}{2}atxx}{x+at}$. This force must be a maximum in the section affording the greatest pressure, and its fluxion must vanish; whence we have $(\frac{1}{2}a^2 - 2bx - atx) \cdot (x+at) = \frac{1}{2}a^2x - a^2b - bx^2 - \frac{1}{2}atx^2$; $(b+\frac{1}{2}at)x^2 + (2abt + a^2t^2)x = a^2b + \frac{1}{2}a^3t$; $x^2 + 2atx = a^2$, $x = \sqrt{(a^2+a^2t^2)} - at$; and if $b=0$, $f = a^2[\frac{1}{2} + t^2 - t\sqrt{(1+t^2)}]$. Hence it appears that, as Mr. Prony has already observed, the angle formed by the surface thus determined, with the vertical surface, is half the complement of the angle of repose, since $\sqrt{(1+t^2)} - t$ is the tangent of half the angle of which the cotangent is t, as is easily shown by a trigonometrical calcula-

tion; and that this angle is independent of the magnitude of the cohesive resistance, and determined only by the friction; at the same time, if the friction vanishes, and the cohesion alone remains, we have $x = a$, the angle being 45°.

B. The portion of a semifluid and cohesive substance, of which the surfaces are horizontal and vertical, affording the greatest lateral pressure, is terminated by a plane.

For if we conceive the substance to be divided by a second vertical surface, parallel to the first, the angular situation of the upper part of the oblique termination, cut off by this surface, will obviously be correctly determined, if considered as a plane, according to the principles already laid down; and if any curved surface would afford a greater lateral pressure than a plane, the direction of the lower part of the oblique termination, considered also as a plane, would require to be different from that of the upper, and this difference might be exhibited by supposing its horizontal extent to be varied, that of the upper portion remaining the same. But in fact, the determination of the direction for this part, thus considered, will be precisely the same as for the upper part; since the proportion of the resistance to the pressure remains the same, and the horizontal force acts on the lower part of the oblique surface with the same increased intensity as the weight, the one depending on the other; so that the relations of all the forces concerned in the determination remain unaltered.

C. To determine what portion of a soft and adhesive substance, having a horizontal and a vertical surface, will stand alone.

Put $f = 0$, then $\frac{1}{2}a^2x - a^2b - bxx - \frac{1}{2}atxx = 0$; and if t is given, let $\sqrt{(1 + t^2)} - t$ be r, and $x = ra$, then $\frac{1}{2}ra^3 - a^2b - r^2a^2b - \frac{1}{2}r^2ta^3 = 0$, and $\frac{1}{2}ra - b - r^2b - \frac{1}{2}r^2at = 0$, and $a = \frac{2b + 2rrb}{r - rrt} = \frac{2b}{r} \cdot \frac{1 + rr}{1 - rt} = \frac{4b}{r}$, and $b = \frac{1}{4}ar$; but if we observe a and x, we find $t = \frac{aa - xx}{2ax}$, and $b = \frac{aax - atxx}{2aa + 2xx}$. When t vanishes, x becomes equal to a, and $b = \frac{1}{4}a$: if $t = 1$, $b = \cdot1036a$: if $t = \frac{1}{2}$, $b = \cdot155a$.

D. When the surface of a soft, or semifluid and cohesive substance, is inclined to the horizon, the portion affording the

greatest horizontal pressure is generally terminated by a curve.

We may suppose the substance to be divided into vertical strata; and the mean depth of any stratum being called y, and the difference of the depths of its two surfaces c, we must inquire what must be its thickness x, in order to afford the greatest horizontal thrust. The weight of the stratum will then be represented by yx; and if the tangent of the elevation of the exposed surface, ascending from its angular end, be u, the length of the oblique termination of the stratum will be $\surd(x^2+(c+ux)^2)=z$: we have then, for the state of equilibrium, the equation $yx \cdot \frac{c+ux}{z} = f \cdot \frac{x}{z} + bz + tyx \cdot \frac{x}{z} + tf \cdot \frac{c+ux}{z}$, and $f = \frac{cyx+uyxx-bzz-tyxx}{x+ct+tux} = \frac{uyx+uyxx-bxx-bcc-2bcux-buuxx-tyxx}{x+ct+tux}$; then putting the fluxion of $f=0$, x only being variable, we obtain $(cy+2uyx-2bx-2bcu-2bu^2x-2tyx) \cdot (x+ct+tux) = (1+tu) \cdot (cyx+uyx^2-bx^2-bc^2-2bcux-bu^2x^2-tyx^2)$; $(2uy-2b-2bu^2-2ty)x \cdot (1+tu)x + (2uy-2b-2bu^2-2ty)x \cdot ct + (cy-2bcu) \cdot (1+tu)x + (cy-2bcu) \cdot ct = (1+tu) \cdot (uy-b-bu^2-ty)x^2 + (1+tu) \cdot (cy-2bcu)x - (1+tu) \cdot bc^2$; and $x^2 + \frac{2ct}{1+tu} \cdot x = \frac{(2bcu-cy) \cdot ct - (1+tu) \cdot bcc}{(uy-b-buu-ty) \cdot (1+tu)}$, whence $\frac{x}{c}$ is found $= \surd\left(\frac{tt}{(1+tu)^2} + \frac{btu-ty-b}{(uy-b-buu-ty) \cdot (1+tu)}\right) - \frac{t}{1+tu}$.

Having thus obtained the angular direction of the termination of the vertical stratum, which affords the greatest lateral thrust when the height is y, we may proceed to find what must be the magnitude of y for different strata, in order that they may all possess this property, and that the whole horizontal force may consequently be the greatest possible. For this purpose we must substitute $\frac{\dot{x}}{-\dot{y}}$ for $\frac{x}{c}$, x being now considered as the whole horizontal thickness, and y the whole vertical ordinate or depth, as before. Hence $-\dot{x} = \frac{\dot{y}}{1+tu}\left(\surd\left(t^2 + \frac{(1+tu) \cdot (btu-ty-b)}{uy-b-buu-ty}\right) - t\right) = \frac{\dot{y}}{1+tu}\left(\surd\frac{-ty-b-btt-t^3y}{uy-b-buu-ty} - t\right) = \frac{\dot{y}}{1+tu}\left(\surd\frac{b+btt+(t+t^3)y}{b+buu+(t-u)y} - t\right)$.

Call $\surd(b+buu+(t-u)\,y)$, v, then $y = \frac{v^2-b-buu}{t-u}$, $\dot{y} = \frac{2v\dot{v}}{t-u}$,

and $-\dot{x} = \frac{2v\dot{v}}{(1+tu)\,.\,(t-u)} \surd\left(b + btt + \frac{(t+t^3)v^2 - b - buu}{t-u}\right) : v - \frac{t\dot{y}}{1+tu} = \frac{2\dot{v}}{(1+tu).(t-u)} \surd\,(b+btt-(b+buu):(t-u)+(t+t^3):(t-u)\,.\,v^2) - \frac{t\dot{y}}{1+tu}$: and if we call $\frac{(t-u)(b+btt)-(b+buu)}{t+t^3}$, d^2, we have $-\dot{x} = \frac{2\dot{v}}{(1+tu)\,.\,(t-u)} \surd\frac{t+t^3}{t-u}\surd(d^2+v^2) - \frac{t\dot{y}}{1+tu}$. But it is well known that the fluent of $\surd\,(a^2+x^2)\,\dot{x}$ is $\frac{1}{2}x\,\surd(a^2+x^2) + \frac{1}{2}a^2\,.\,\text{HL}\,(x + \surd(a^2+x^2))$, and by comparison with this fluent, we obtain the equation $e - x = \frac{1}{(1+tu).(t-u)} \surd\frac{t+t^3}{t-u}\,(v\,\surd(d^2+v^2) + d^2\,\text{HL}\,(v + \surd\,(d^2+v^2))) - \frac{ty}{1+tu} + c$. When, however, $t-u$ is negative, that is, when the elevation of the inclined surface is greater than could exist without the cohesion, the fluent assumes a different form, and we must make $d^2 = \frac{(u-t).(b+tt)+b+buu}{t+t^3}$; then $-\dot{x} = \frac{-2\dot{v}}{(1+tu).(u-t)} \surd\frac{t+t^3}{u-t}\surd\,(d^2-v^2) - \frac{t\dot{y}}{1+tu}$. But it is known that the fluent of $\surd\,(a^2-x^2)\,\dot{x}$ is $\frac{1}{2}x\,\surd(a^2-x^2) + \frac{1}{2}a^2$ arc sine $\frac{x}{a}$; hence $e-x$ becomes $= e - \frac{1}{(1+tu).(u-t)}\cdot\surd\frac{t+t^3}{u-t}\left(d\,\surd\,(d^2-v^2) + d^2 \text{ arc sine } \frac{v}{d}\right) - \frac{ty}{1+tu}$.

E. When the variable resistance vanishes, the curve becomes a parabola.

For if $t=0$, $\frac{x}{c}$ or $\frac{\dot{x}}{-\dot{y}}$ becomes $= \surd\frac{b}{b+buu-uy}$, whence $x+e = \frac{2}{u}\surd\,(b^2+b^2u^2-buy)$; but when $x=0$, $y=a$, and $e = \frac{2}{u}\,\surd\,(b^2+b^2uu-bau)$, and therefore $(x + \frac{2}{u}\surd(b^2+b^2uu-bau))^2 = \frac{4bb}{uu} + 4b^2 - \frac{4b}{u}y = x^2 + \frac{4bb}{uu} + 4b^2 - \frac{4ba}{u} + \frac{4x}{u}\surd(b^2+b^2uu-bau)$. and $y = a - \surd\,(1+uu-\frac{a}{b}u)\,x - \frac{u}{4b}x^2$. In order to determine the whole horizontal force, we must find its fluxion by substituting $\dot{x}$ for x, and $-\dot{y}$ for c, in the equation for f, which becomes $-y\dot{y} + uy\dot{x} - b\dot{x} - b\frac{\dot{y}\dot{y}}{\dot{x}} + 2bu\dot{y} - bu^2\dot{x}$; and since $-\dot{y} = \surd\,(1+uu-\frac{a}{b}u)\,\dot{x} + \frac{u}{2b}x\dot{x}$, we obtain the fluent

$= g - \frac{1}{2}y^2 + aux - \frac{u}{2}\sqrt{(1 + uu - \frac{a}{b}u)}\, x^2 - \frac{uu}{12b}x^3 - bx - bx -$ $bu^2x + aux - \frac{u^3}{12b}x^3 - \frac{u}{2}\sqrt{(1 + uu - \frac{a}{b}u)}\, x^2 + 2buy - bu^2x =$ $g + (2au - 2b - 2bu^2)x - u\sqrt{(1 + u^2 - \frac{a}{b}u)}\, x^2 - \frac{uu}{6b}x^3 + 2buy - \frac{1}{2}y^2$, which must vanish when $x = 0$, and $y = a$, or $g + 2bau - \frac{1}{2}a^2 = 0$, and $g = \frac{1}{2}a^2 - 2bau$. When $y = 0$, $x + e = \frac{2b}{u}\sqrt{(1 + u^2)}$, and $x = \frac{2b}{u}\sqrt{(1 + u^2)} - \frac{2b}{u}\sqrt{(1 + u^2 - \frac{a}{b}u)}$, and the whole force is $\frac{1}{2}a^2 - 2bau + (2au - 2b - 2bu^2)x - u\sqrt{(1 + u^2 - \frac{a}{b}u)}\, x^2 - \frac{uu}{6b}x^3$.

Here it must be observed that when $\frac{a}{b}u$ is equal to or greater than $1 + u^2$, the problem becomes impossible, the value of $\frac{x}{c}$ becoming first infinite and then imaginary. We may take for an example the case $u = \frac{1}{10}$ and $a = 10b$, then $x = 2a(\sqrt{1{\cdot}01} - {\cdot}1) = 1{\cdot}81a$, and the whole force is $\frac{1}{2}a^2 - {\cdot}02a^2 + ({\cdot}2a - {\cdot}2a - {\cdot}002a)x - {\cdot}01x^2 - \frac{x^3}{60a} = {\cdot}345a^2$. If $u = 1$, and $a = 2b$, $x = \sqrt{2}a$, and the force $\frac{1}{2}a^2 - a^2 - \sqrt{2}a^2 - \frac{2}{3}\sqrt{2}a^2$, which being negative, implies that there can be no separation. In order to show how little the force thus determined differs from that which is afforded by a section terminated by a plane surface, even where the variable resistance is supposed to be absent, we may calculate, for the depth of a, the horizontal extent x of a prismatic section affording the greatest pressure, the equation of the forces will then be $\frac{1}{2}ax\frac{a+ux}{z} - bz = f \cdot \frac{x}{z}$, and $f = \frac{1}{2}a^2 + \frac{1}{2}aux - \frac{bz^2}{x} = \frac{1}{2}a^2 + \frac{1}{2}aux - bx - \frac{ba^2}{x} - 2bau - bu^2x$, and when its fluxion vanishes, $\frac{1}{2}au - b + \frac{baa}{xx} - bu^2 = 0$, consequently $\frac{aa}{xx} = 1 + u^2 - \frac{au}{2b}$, which, when $u = \frac{1}{10}$, and $a = 10b$, becomes ${\cdot}51$, and $x = 1{\cdot}4a$, whence f is found ${\cdot}337a^2$, which is not one-fortieth part less than the more correct result of the former calculation. When the cohesion vanishes, and the variable resistance alone remains, the maximum of force seems in all cases to be afforded by a plane surface, whether the resistance is horizontal or not.

F. It remains to be determined, what is the proportion of the forces, when the pressure, instead of being horizontal, is supposed to be oblique, as will be the case when the surface of a wall is opposed to the thrust of earth, and exhibits a lateral adhesion or friction, as well as a direct resistance.

We have here two new forces to be considered, the one constant, representing the adhesion of the wall, the other depending on f the horizontal pressure, both tending directly to lessen the weight, if we consider the surface of the wall as vertical. We may still call the horizontal extent of the prismatic portion x, disregarding the slight inaccuracy of supposing the oblique surface a plane; and u being, as above, the tangent of the elevation of the exposed surface, the friction of the wall being, for the sake of simplicity, considered as equal to the internal friction of the materials, which it can never exceed, and of which it will seldom fall short, we have the equation $(\frac{1}{2}ax - ab - tf)\frac{a+ux}{z} - bz - t(\frac{1}{2}ax - ab - tf)\frac{x}{z} - tf\frac{a+ux}{z} = f\frac{x}{z}$, and $f = \frac{(\frac{1}{2}ax-ab).(a+ux)-bz^2-tx(\frac{1}{2}ax-ab)}{t(a+ux)-ttx+t(a+ux)+x}$; and when its fluxion vanishes, $(\frac{1}{2}a^2 + aux - abu - 2bx - 2abu - 2bu^2x - atx + tab)$. $(2t(a+ux) - t^2x + x) = (2tu - t^2 + 1)$. $[(\frac{1}{2}ax - ab) . (a+ux) - bx^2 - b(a+ux)^2) - \frac{1}{2}tax^2 + tabx]$, or $[(au - 2b - 2bu^2 - at)x + \frac{1}{2}a^2 - 3abu + atb]$. $[(2tu - t^2 + 1)x + 2at] = (2tu - t^2 + 1)$. $[(\frac{1}{2}au - b - bu^2 - \frac{1}{2}at)x^2 + (\frac{1}{2}au - abu - 2abu + atb)x - 2a^2b]$; whence $x^2 + \frac{2atx}{2tu - tt + 1} + \frac{2at(\frac{1}{2}a^2 - 3abu + atb)}{(au - 2b - 2buu - at).(2tu - tt + 1)} = \frac{1}{2}x^2 - \frac{2a^2b}{au - 2b - 2buu - at}$, or $x^2 + \frac{4atx}{2tu - tt + 1} = 2a^2 . \frac{2btu - at - 2b}{(2tu - tt + 1).(au - 2b - 2buu - at)}$, and x may be found by completing the square.

But for practical use on a large scale, we may neglect the cohesive resistance without impropriety, its value being generally variable, from the effects of moisture and agitation, so that it would be unsafe to place any dependence on it, even if it were much larger than commonly happens: we may therefore make $b = 0$, and $x^2 + \frac{4atx}{2tu - tt + 1} = 2a^2\frac{t}{(2tu - tt + 1).(t - u)}$,

and $x + \frac{2at}{2tu - tt + 1} = \pm a \sqrt{\left(\frac{4t^2}{(2tu - tt + 1)^2} + \frac{2t}{(2tu - tt + 1) . (t - u)}\right)} = \pm \frac{a}{2tu - tt + 1} \sqrt{\frac{2t + 2t^3}{t - u}}$; whence f may be readily determined, being equal to $\frac{1}{2}ax . \frac{a - (t - u)x}{2at + 2tux - ttx + x}$.

If the wall, instead of being vertical, be inclined towards the bank, which is a condition highly favourable to its stability, the oblique direction of the thrust must also be taken into consideration, in computing its magnitude. Let u be now the tangent of the deviation of the wall from the vertical direction, the surface of the earth being horizontal, and let x be, as above, the whole horizontal extent of the portion affording the greatest thrust, the force f being perpendicular to the wall. We shall then have for the weight, $\frac{1}{2}a\,(x - au)$, acting in the direction of the oblique surface z with the force $\frac{1}{2}a\ (x - au)\,\frac{a}{z}$, and causing a resistance $\frac{1}{2}at(x - au)\frac{x}{z}$. In order to reduce the force f to the same direction, we must find the sine and cosine of the angle contained by the oblique surface z and the wall, which are $\frac{x - au}{\sqrt{(1 + uu)}}$, and $\sqrt{\left(1 - \frac{(x - au)^2}{z^2 + u^2 z^2}\right)} = \frac{1}{z}\sqrt{\left(a^2 + x^2 \frac{(x - au)^2}{1 + u^2}\right)} = \frac{a + ux}{z\sqrt{(1 + uu)}}$; whence we have $f\frac{x - au}{z\sqrt{(1 + uu)}}$, and $tf\frac{a + ux}{z\sqrt{(1 + uu)}}$; and the friction of the wall, ft, being reduced in a similar manner, gives $ft\,\frac{a + ux}{z\sqrt{(1 + uu)}}$, and $-ft^2\frac{x - au}{z\sqrt{(1 + uu)}}$, whence we have the equation $\frac{1}{2}a\,(x - au)\,\frac{a}{z} = \frac{1}{2}at\,(x - au)\,\frac{x}{z} + f\,\frac{x - au}{z\sqrt{(1 + uu)}} + ft\,\frac{a + ux}{z\sqrt{(1 + uu)}} + ft\,\frac{a + ux}{z\sqrt{(1 + uu)}} - ft^2\,\frac{x - au}{z\sqrt{(1 + uu)}}$, or $a^2\,(x - au) = atx\,(x - au) + 2f\,\frac{x - au}{\sqrt{(1 + uu)}} + 4ft\,\frac{(a + ux)}{\sqrt{(1 + uu)}} - 2ft^2\,\frac{x - au}{\sqrt{(1 + uu)}}$; consequently $\frac{f}{a\sqrt{(1 + uu)}} = \frac{(a - tx)\,.\,(x - au)}{(2 - 2tt)\,.\,(x - au) + 4t\,(a + ux)}$: this we may call $\frac{b + cx + dxx}{ex + g}$; and when its fluxion vanishes, $(c + 2dx)\,.\,(ex + g) = eb + cex + dex^2 = cex + 2dex^2 + cg + 2dgx$, $x^2 + \frac{2g}{e}x = \frac{b}{d} - \frac{cg}{de}$, and $x = \sqrt{\left(\frac{b}{d} - \frac{cg}{de} + \frac{gg}{ee}\right)} - \frac{g}{e}$. Here $b = -\,a^2u$, $c = a + atu$, $d = -\,t$, $\frac{b}{d} = \frac{au}{t}$, $\frac{c}{d} = -\,\frac{a}{t} - au$, $e = 2 - 2t^2 + 4tu$, and $g = 4at - (2 - 2tt)\,au$.

G. It will now be easy to find the dimensions of a wall, capable of withstanding the thrust of a given bank of earth, without being overturned or carried away horizontally, provided that we know the elevation at which the surface of the earth is capable of supporting itself.

It is obvious that the whole pressure, like that of fluids, must be proportional to the square of the depth a, neglecting the effect of adhesion; and consequently that the centre of pressure must be at one-third of the height. We may consider the specific gravity of the wall as equal to that of the earth, which will in general allow us some excess of stability for the security of the work: then if the wall be vertical, and its thickness be y, the force being referred to the outside of the base of the wall as the fulcrum of a lever, we must have, in order that it may not be overturned, $\frac{1}{3}af = tfy + \frac{1}{2}ayy$, and $y^2 + \frac{2ft}{a}y = \frac{2}{3}f$, $y = \sqrt{(\frac{2}{3}f + \frac{f^2t^2}{a^2})} - \frac{ft}{a}$. And in the same manner, if we suppose the section of the wall to be triangular, its outer surface being sloped off, we have $\frac{1}{3}af = tfz + \frac{1}{4}azz$, and $z = \sqrt{(\frac{4}{3}f + (\frac{4f^2t^2}{a^2})} - \frac{2ft}{a}$, z being the thickness at the bottom. When the wall is inclined towards the bank, in an angle of which the tangent is u, f being the force perpendicular to it, and y the horizontal thickness of the wall, the force f will act on a lever of which the length is $\frac{2}{3}a\sqrt{(1+uu)} + \frac{uy}{\sqrt{(1+uu)}}$, and the friction tf will act at the distance $\frac{y}{\sqrt{(1+uu)}}$, and the weight at $\frac{1}{2}y + \frac{1}{2}au$, whence $\frac{1}{3}af\sqrt{(1+uu)} + \frac{fuy}{\sqrt{(1+uu)}} = \frac{fty}{\sqrt{(1+uu)}} + \frac{1}{2}ay^2 + \frac{1}{2}a^2uy$, and $y^2 + (\frac{2ft-2fu}{a\sqrt{(1+uu)}} + au)\,y = \frac{2}{3}f\sqrt{(1+uu)}$; consequently $y = \sqrt{[\frac{2}{3}f\sqrt{(1+uu)} + (\frac{ft-fu}{a\sqrt{(1+uu)}} + \frac{1}{2}au)^2]} - \frac{ft-fu}{a\sqrt{(1+uu)}} - \frac{1}{2}au$.

If the wall be not securely fixed at its foundations, for example when the earth is dug away beyond it, it may be liable to slide away laterally more easily than to be overturned. Supposing it simply to rest on materials similar to those which constitute the bank, we may calculate the thickness sufficient to produce a resistance equivalent to the thrust; thus if the wall

is vertical, we must have $f = t\,(ay + ft)$, and $ay = \frac{f}{t} - ft$; but when the wall is inclined, the force f takes from the weight the portion $f\,\frac{u}{\sqrt{(1+uu)}}$, and the friction adds to it only $ft\,\frac{1}{\sqrt{(1+uu)}}$, the horizontal thrust being $f\frac{1}{\sqrt{(1+uu)}}$; whence $f\,\frac{1}{\sqrt{(1+uu)}} = t\,(ay - f\,\frac{u}{\sqrt{(1+uu)}} + f\,\frac{t}{\sqrt{(1+uu)}}$, and $ay = \frac{f}{\sqrt{(1+uu)}}\left(\frac{1}{t} + u - t\right)$.

H. In the case of driving a pile, the pressure of the soft materials is modified by the inversion of the direction of the friction of the vertical surface, which now acts in conjunction with the weight of the materials, so that $\frac{1}{2}ax - ab - tf$ becomes $\frac{1}{2}ax + ab + tf$, or, if $b = 0$, simply $\frac{1}{2}ax + tf$; and $f = \frac{1}{2}\cdot\frac{aa - atx}{tt+1}$, which is greatest when x is least, and becomes ultimately $\frac{\frac{1}{2}aa}{tt+1}$, and the resistance tf will be $\frac{1}{2}a^2\,\frac{t}{tt+1}$, which is a maximum when $tt + 1 = 2t$, or $t = 1$, being then $\frac{1}{4}a^2$; and in this case the resistance derived from the friction, on the whole of the lateral surfaces of a square pile, would be equal to the weight of the earth which would press on one of the surfaces, if it were buried at the depth to which its lower end has penetrated. There would however be other resistances from the tenacity preventing the ready separation of the earth before the pile, which would perhaps considerably exceed the friction thus determined.

I. Such of the results of these calculations, as are most likely to be of practical utility, may be conveniently exhibited in the form of a table: but it must be remembered, in its application, that some additional strength ought always to be given to the works concerned, in order to ensure their stability, and that occasional agitation will very much diminish the resistance of almost all kinds of materials; to say nothing of the precaution necessary to obviate the effects of the penetration of water; which will not only act by its own hydrostatic pressure, but also weaken the adhesion of the earth employed, unless a sufficient number of apertures can be provided for allowing it to escape.

TABLE OF THE THRUST OF EARTH AGAINST AN UPRIGHT WALL.

Surface Horizontal.

Tangent of the angle of repose, expressing the proportion of the resistance to the pressure.	Angle of repose at which the substance will support itself.	Breadth of the surface of the portion affording the greatest pressure, the height being unity.	Horizontal thrust, that of a fluid being unity.	Requisite thickness of a vertical wall of equal specific gravity, its height being unity.	Requisite thickness of triangular wall; the external surface being oblique.	Thickness required to secure a wall from sliding.
1 : ∞	00° 00′	(1·414)	1·000	·577	·707	∞
1 : 10	5 43	1·234	·761	·491	·591	3·767
1 : 8	7 7	1·194	·713	·470	·575	2·812
1 : 6	9 28	1·132	·640	·444	·539	1·867
1 : 5	11 18	1·086	·589	·424	·514	1·414
1 : 4	14 2	1·022	·522	·396	·479	·979
1 : 3	18 26	·927	·430	·355	·430	·573
1 : 2	26 34	·774	·300	·292	·352	·225
2 : 3	33 41	·660	·217	·246	·295	·090
3 : 4	36 52	·611	·186	·226	·270	·059
1 : 1	45 0	·500	·125	·184	·221	·000
		Descent of the surface towards the wall				10°
1 : 5	11 18	3·750	·805	·491	·591	
1 : 3	18 26	1·518	·528	·399	·481	
1 : 2	26 34	1·058	·373	·322	·386	
3 : 4	36 52	·753	·211	·240	·287	
1 : 1	45 0	·585	·138	·192	·230	
		Descent of the surface towards the wall				20°
1 : 2	26 34	2·022	·452	·350	·418	
3 : 4	36 52	1·040	·247	·255	·304	
1 : 1	45 0	·743	·155	·200	·239	
						30°
3 : 4	36 52	1·581	·302	·282	·335	
1 : 1	45 0	1·076	·186	·220	·262	
						40°
1 : 1	45 0	1·713	·246	·248	·294	
		Ascent of the surface towards the wall				10°
1 : 5	11 18	·746	·507	·391	·478	
1 : 3	18 26	·712	·374	·332	·401	
1 : 2	26 34	·639	·267	·276	·333	
3 : 4	36 52	·534	·170	·217	·261	
1 : 1	45 0	·452	·119	·180	·216	
						20°
1 : 2	26 34	·559	·239	·263	·317	
3 : 4	36 52	·486	·154	·208	·250	
1 : 1	45 0	·377	·117	·179	·214	
						30°
3 : 4	36 52	·460	·133	·195	·234	
1 : 1	45 0	·408	·098	·165	·197	
						40°
1 : 1	45 0	·408	·090	·159	·203	

TABLE OF THE THRUST OF EARTH AGAINST A WALL INCLINED TOWARDS THE BANK IN AN ANGLE OF 11° 18′, OF WHICH THE TANGENT IS 2; THE SURFACE BEING HORIZONTAL.

Tangent of the angle of repose.	Angle of repose.	Horizontal extent of the portion affording the greatest pressure.	Direct thrust against the wall, that of a fluid being unity.	Horizontal thrust.	Thickness required to secure a wall from being overturned.	Thickness required to secure a wall from sliding back.
1 : 00	00° 00′	(·200)	1·000	·500	·540	∞
1 : 10	5 43	1·419	·721	·360	·440	3·640
1 : 8	7 7	1·373	·661	·330	·410	2·670
1 : 6	9 28	1·296	·579	·289	·367	1·747
1 : 5	11 18	1·242	·523	·261	·337	1·308
1 : 4	14 2	1·166	·452	·226	·300	·847
1 : 3	18 26	1 059	·354	·177	·248	·508
1 : 2	26 34	·885	·225	·112	·176	·191
2 : 3	33 41	·733	·149	·074	·130	·077
3 : 4	36 52	·636	·121	·060	·111	·047
1 : 1	45 0	·586	·071	·035	·074	·007

An instance has occurred on a large scale, where the wall of a dock has given way horizontally, when its mean thickness was about ·230, the ground having been dug away beyond its foundation: it was of brick, and somewhat curved, being vertical at the top, while the inclination of the chord, or the mean inclination, was 11° 18′, as is supposed in the second table. Hence it appears that the friction must have been somewhat less than $\frac{1}{2}$ of the weight, and that the materials would have stood at an angle of about 25°; to have overturned this wall, the materials must have exhibited a friction of about one-third of the weight, and have been incapable of standing at a greater inclination than about 20°.

In general, it will be unquestionably proper to calculate on a friction not exceeding $\frac{1}{2}$ of the weight, and to make the thickness of a wall, if vertical, at least $\frac{3}{10}$ or perhaps $\frac{1}{3}$ of its height, and if inclined in an angle of 10° or 12°, about $\frac{1}{5}$, taking care to secure the foundation from sliding, to which an inclined wall will otherwise be liable if its thickness be less than $\frac{1}{4}$, though a vertical wall would be safe in this respect if its thickness were sufficient to secure it from being overturned. The disposal of a part of the materials of the wall in the form of counterforts, or

buttresses, will add to the strength in either case, especially with respect to the danger of overturning: the curvature, which is a considerable convenience in the case of a dock, tends in a slight degree to lessen the stability with respect to sliding, and makes it still more necessary to attend to the security of the foundation. On the other hand, when we have an opportunity of ascertaining, by a simple experiment, the utmost fluidity that can be communicated by accidental moisture to a chalky or gravelly soil, these calculations may often justify us in saving a very great expense, by proportioning the strength of the works to the object required to be attained by them.

No. L.

REMARKS ON

THE STRUCTURE OF COVERED WAYS

INDEPENDENT OF THE PRINCIPLE OF THE ARCH IN EQUILIBRIUM, AND ON THE BEST FORMS FOR ARCHES IN BUILDINGS.*

From Nicholson's Journal for 1807, vol. xviii. p. 241.

To MR. NICHOLSON.

SIR,

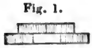
THE subterraneous passages or tunnels of the Babylonians, and perhaps the cloacæ of the Romans, were constructed, according to the opinion of the best informed antiquaries, by simply causing the bricks or stones of each of the side walls to project more and more as they rose higher, till they finally met in the summit. The most ancient remains of the Grecian buildings, for example, the treasury of Atreus at Mycenæ, and other ruins in the Peloponnesus, exhibit in general over their doors, according to the reports of modern travellers, a triangular aperture, formed by large stones; the base of the triangle coinciding with the lintel of the door; and the pointed arches of the Gothic buildings are by no means universally so arranged, as to derive their stability from the proportion of their curvature in every part, to the pressure which would be produced, according to the commonly received theory, by the height of the superincumbent wall. As far as I know, this

* This article is somewhat remarkable, at least amongst Dr. Young's writings, for its clear and elegant geometrical treatment of the subject of arches under their most simple conditions, and leads to some very curious results. The article on Bridges, which follows next but one to it, will involve considerations of a much more recondite nature, where the friction of the materials and their resistance to extension and compression, as well as the mechanical equilibrium of the arches which they form, will be taken into account.—*Note by the Editor.*

subject has not been mathematically investigated in all its parts, and I shall therefore submit to the consideration of your readers some propositions relating to the stability of overhanging walls and of triangular covered ways.

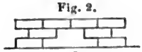
I shall examine those cases only, in which the materials employed are equal rectangular parallelopipeds, whether bricks or wrought stones, and in the first place I shall suppose them destitute of all friction or adhesion, and placed horizontally. With such materials, it may be shown from the principles of the lever only, that a covered way may easily be made, not exceeding in breadth the length of three or four bricks or stones, and that the combinations, represented in Figs. 1 . . 7,

Fig. 1.



1

Fig. 2.



2

Fig. 3.

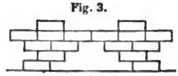


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Fig. 4.

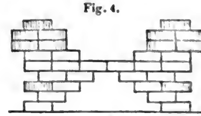

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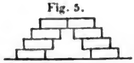
Fig. 5.



1½

Fig. 6.

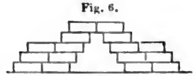


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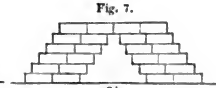
Fig. 7.



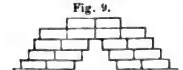
2½

will stand in equilibrium without external support: and that if the breadth of the way be equal only to the length of two bricks, it may have any height of wall added over it without destroying the equilibrium (Fig. 8). These combinations are however incapable of resisting the pressure of any considerable force, and the method of building in this manner cannot be generally advisable; but the weight of two bricks is supported at the vertex in Fig. 9, and by extending the basis, and heightening the wall at the sides, a much greater strength might be obtained. It is however obvious, that a wall terminated in this manner would by no means necessarily exert such a pressure

Fig. 8.

2

Fig. 9.



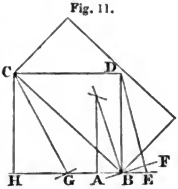
2

on any stones forming a facing of the oblique surface, as is commonly supposed in the theory of the arch; on the contrary it is plain, that an arch might be turned under it, which would be sufficiently strong for every purpose, if capable of supporting little more than its own weight: and the same reasoning is applicable to the wall in contact with the lower parts of every common arch. Hence it becomes often eligible to construct the arch in such a manner as to be more capable of resisting a pressure near its vertex; and thus its form will approach in some degree to that of a pointed arch. The arches of bridges, on the contrary, have to support the pressure of materials of a very different description; and for this reason their greatest curvature should be near the abutments.

In the next place I shall inquire into the conditions requisite for the stability of an oblique facing, composed of rectangular bricks or stones only, both with and without the consideration of the effects of friction. The simplest case that can be proposed is that of two bricks meeting each other, and standing on a perfectly smooth and horizontal plane, the centre of gravity of each being vertically above the lowest angle (Fig. 10). But if the base be widened, the surfaces supporting the bricks must be rendered oblique. The weight of the brick acts on a lever of which the length is A B (Fig. 11), in turning it round the point B; and this is resisted by the horizontal thrust at C acting on the lever B D, hence the horizontal thrust must be to the weight as A B to B D, and making B E = A B, the horizontal thrust at B combined with the weight will act in the direction D E, and the brick will be supported by a surface B F perpendicular to D E. Supposing the thickness of the brick inconsiderable, the centre of gravity being in the line B C, taking B G half B H, the line C G will be perpendicular to the surface on which it will rest in equilibrium; and this theorem may be of considerable use in carpentry, for finding the best possible direction for the abutment of a rafter. If the abutment is in the direction of the end of

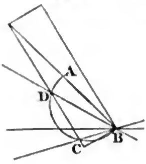
Fig. 10.

Fig. 11.



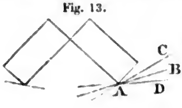
the block Fig. 12, describe on half the diagonal, A B, the semicircle B C D A; and C B, or D B, will show the position of a line, which being made horizontal, the block will be supported in equilibrium. If the horizontal line cross the circle between C and D, the end B will slide downwards, but if between A and D, or B and C, it would be urged upwards, but the bearing would be transferred to the lower corner, and the whole will remain at rest: and this will be the case in all positions, when the circle falls wholly within the side of the block, that is, when its thickness is not much less than half its length. Thus two common bricks would remain firm in all elevations if placed with the narrow sides of their ends lowermost; even without any friction: but with the wider sides lowermost, they would slide down the abutments if the distance of their ends were more than about two, and less than fourteen inches.

Fig. 12.



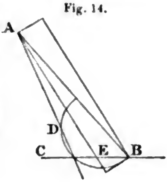
The last additional circumstance which requires to be examined, with regard to the stability of bricks or stones in oblique situations, is the effect of friction or adhesion. This force may be considered, in all practical investigations, as proportional to the mutual pressure of the surfaces concerned; and the most convenient way of estimating its magnitude is to incline the surfaces to the horizon, until they begin to slide on each other. The angle at which this happens will be always very nearly if not exactly the same for surfaces of the same kind, and it may with propriety be called the angle of repose; and it is obvious, that any other force acting on the surface in the same angle as that in which the force of gravity acts in this instance will be completely obviated by the resistance of the surface: and the friction will be to the pressure, as the tangent of the angle of repose to the radius. If therefore the surface A B (Fig. 13) is calculated to resist the pressure of the block A without friction, by making the angles B A C and B A D each equal to the angle of repose, we may determine the greatest and least inclination

Fig. 13.



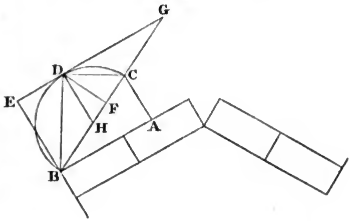
which will be sufficient for retaining the block by the assistance of the friction or adhesion; the stability being greatest of all in the original situation A B. In the same manner the rectangular block A B (Fig. 14) will be supported by its abutment as long as the horizontal line BC crosses the semicircle within the line A D, D A E being equal to the angle of repose.

Fig. 14.



When two blocks of equal dimensions form an overhanging facing on each side of a triangular aperture (Fig. 15), the upper one is in the same predicament as if it rested simply on a fixed abutment; the lower one is retained in its situation by the force of friction only. If A B C be the angle of repose, the direction of the force supporting each of the upper blocks will be B C; and if the vertical line B D represent the weight of the block A, B C will be the resisting force, and A C the friction, which counteracts the tendency of the block B to descend along the abutment, and this force is represented by E B. In order therefore to find the position in which the block B will most readily slide away, we must make the proportion of E B to A C a maximum; and this will happen when the mean of the angles D B A and D B C is equal to half a right angle. For the sine of the angle DBC being $\frac{DC}{BC}$, and its cosine $\frac{BD}{BC}$, and the sine and cosine of A B C being $\frac{AC}{BC}$, and $\frac{AB}{BC}$, the sine of D B A is $\frac{DC.AB}{BCq}+\frac{BD.AC}{BCq}$, and consequently $EB = BD.\frac{DC.AB+BD.AC}{BCq}$, which, divided by A C, is $BD.\frac{DC.AB}{AC.BCq}+\frac{BDq}{BCq}$, and this must be a maximum, consequently, BC being supposed constant, $\frac{AB}{AC}$.

Fig. 15.



$\frac{BD.DC}{BC} + \frac{BDq}{BC}$ must also be a maximum. Then if we make D F perpendicular to B C, and the angle F D G = A C B, D F will be $\frac{BD.DC}{BC}$, $FG = \frac{AB}{AC} \cdot \frac{BD.DC}{BC}$, and $BF = \frac{BDq}{BC}$, so that B G must be a maximum, which will evidently happen when DG is a tangent to the semicircle BDC, and the angle DBC half D H C, which is the difference between A B C and a right angle. If we wish to determine the proportion of the friction to the pressure when the friction is barely capable of retaining the block in its situation in the most unfavourable position, let x be the sine, and y the cosine of half the angle A B C, then the sine and cosine of half a right angle being $\sqrt{\frac{1}{2}}$, the sine of A B D or B D E, as well as that of B C D, will be $\sqrt{\frac{1}{2}}\, x + \sqrt{\frac{1}{2}}\, y$. Now, if the weight be B D, $BC = \frac{BD}{(x+y)\sqrt{\frac{1}{2}}}$, and the sine of A B C being $2xy$, A C is $\frac{2xyBD}{(x+y)\sqrt{\frac{1}{2}}}$: but the weight which produces the friction is three times the weight of a single block, the friction on the upper surface being derived from the pressure of the highest block, and that on the lower from the pressure of both blocks; while the tendency to descend belongs to the lower block only, and is therefore expressed by $BD . \sqrt{\frac{1}{2}} \times (x+y)$; hence we have the equation $(x+y)\sqrt{\frac{1}{2}} = \frac{6xy}{(x+y)\sqrt{\frac{1}{2}}}$; therefore $\frac{1}{2}(x+y)^2 = 6xy$, $(x+y)^2 = 12xy$, $x^2+y^2 = 10xy = 1$, and $2xy = \frac{1}{5}$, which is the sine of A B C, and the friction is in this case to the oblique resistance as 1 to 5, and to the pressure nearly as 10 to 47: so that whenever the friction is greater than this, which is almost always the case with the materials commonly employed, two pairs of equal blocks meeting each other in this manner will be secure from sliding in every possible position. If there are more than two blocks on each side, or if the lower blocks are larger than the upper one, the force tending to support the lower ones, which is derived from the pressure of the upper one, is twice the immediate friction occasioned by its weight, since the same pressure acts in two different places, and as long as this exceeds the difference between the friction and the relative weight of the lower block or blocks, they will be secure from sliding along

the abutments. For example, in the case of common bricks or stone, the friction is at least half of the pressure; for if a brick be placed with the short side of its end downwards on another which is gradually raised, it will fall over before it slides; we may therefore safely estimate the friction as equal to half the pressure, the tangent of the angle A B C being .5, its sine .446, and its cosine .892. Now if the whole aperture be supposed equilateral, the sine of D B A will be .5, and its cosine .866; and the sine of D B C nearly .06; and the friction A C will be to the weight B D as .45 to 1, and to E B as 9 to 10, so that 18 bricks on each side might be secured from sliding by the double effect of the upper pair.

There are however two other ways in which such a structure might give way: the lower portion revolving on its lowest point, and the higher either moving with it towards the opposite side, or sliding upwards in a contrary direction: and in order that the pile may stand, it is obvious that it must possess sufficient stability in both these respects. When there are only two equal blocks on each side, it is easy to determine whether or no their breadth is sufficient to prevent their both falling inwards by describing round the triangle A B C (Fig. 16), a segment of a circle, by making D E vertical, and joining A E, which must either coincide with the diagonal A F, or be below it. If there are more than two pieces on each side, in order to determine the stability of any joint A B (Fig. 17), let A C and D E be horizontal, and F E vertical, draw D B C, make E H = E G, and H I horizontal and equal to half A C; then if F I fall below B, the structure will not give way at the joint A B. The demonstration may easily be deduced from the principle of the equality of the horizontal thrusts in the case of an equilibrium: and it may be shown that, if the aperture be equilateral, 15 common bricks on each side will stand, but 16

Fig. 16.

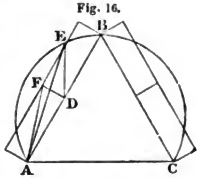
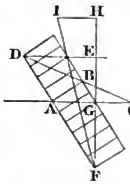
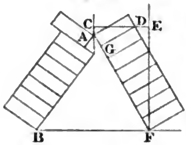


Fig. 17.



will give way at the sixth joint from the summit. The stability is however less considerable with respect to the second mode of failure, in which the upper brick slides outwards, while all below it fall inwards (Fig. 18). In this case the angular motion of the two portions is initially equal, the points A and B remaining fixed. The velocities of the centres of gravity reduced to a vertical direction are as the distances C D, D E; in order therefore that there may be an equilibrium without friction, the weight of the upper portion must be to that of the lower as D E to C D; and in all cases the force of A D, tending to support D F, is to the weight of D F, acting at its centre of gravity, as A G . C D to F G . D E, or as A G . $\frac{\text{C D}}{\text{D E}}$ to F G. The friction of the upper block, of which the magnitude may be determined in the manner already shown, will act upon the whole length of the arm F G, while the weight of D F acts only on the length of half D E, consequently its effect must be considered as increased in the ratio of D E to twice F G. Thus if we take the example of an equilateral aperture, constructed with 8 common bricks on each side, and without cement of any kind, C D will be 9·3 inches, D E 2·7, and F G 21; hence the brick A will produce immediately a force equivalent to the weight of 3·4 bricks, and by its friction, which is $\frac{9}{10}$ of its weight, another force equivalent to the weight of 7 more; consequently the sum of both will be fully adequate to the support of the 7 bricks which form the lower portion of the structure. But if we make the same calculation for 9 bricks, we shall find that they will not stand without some external support.

Fig. 18.



It is obvious that in all these cases the addition of any load at the summit of the structure would very materially increase its stability, and that even a block, of sufficient magnitude to fill up the angle only, would enable us considerably to extend the base. It is also plain that an inclined facing of this kind is not distinguished from an arch by the want of a key stone, since the two middle blocks act nearly in the same manner as if they were united, except when they are forced outwards by

the pressure of the lower parts; and a centre is as necessary for raising a facing of this kind, as if it were an arch of any other form.

I am, Sir,

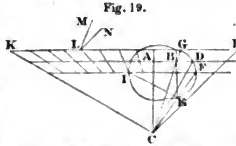
Your very obedient servant,

APSOPHUS.

17 Oct., 1807.

Postscript.—The equilibrium of the flattened arches, commonly placed over windows, may be determined in a similar manner, the principles being the same as those which are employed in the construction of Fig. 11 and Fig. 13. Supposing the blocks without friction and of equal height, if their divisions converge to one point, the lateral thrust will be equal throughout, and the whole will remain in equilibrium, provided that the ends do not slide outwards. In order to find the breadth which is within this limit, let the horizontal line A B (Fig. 19) pass through the centre of gravity of the blocks, draw any line C B from the centre of divergence C, make B D = A B, join C D, and let the vertical line B E meet it in E; then E F, drawn to the intersection of the semicircle E F G with the lower termination of the blocks, will show the direction of the abutment, which will afford an equilibrium: and C H parallel to it will determine the greatest breadth that will stand. But since the blocks thus disposed, and supporting a wall, cannot slide away without displacing the superincumbent weight, the whole wall may be considered as adding to the height of the blocks, and the stability in every case that can occur in practice, must be complete: it is only necessary to reduce the horizontal thrust as much as possible, and this must be done by making the point C as near the blocks as convenient: the thrust being equal to the weight of the portion A H, supposing A C H half a right angle. If we wish to estimate also the effects of friction, let the segment E I G contain a right angle, diminished by the angle of repose, then C K, parallel

Fig. 19.



to E I, will be the direction of the abutment which will secure the blocks from sliding outwards, with the assistance of the force of friction. Generally however the obliquity must be much less than this; and the resistance of the abutment becomes capable of being exerted in the most favourable direction that its friction will allow, that is, in a direction more nearly vertical than the perpendicular to its surface, for example L M, M L N being the angle of repose; and if we wish to have the thrust equal throughout, we must employ blocks of such a form that their divisions may make, with the lines converging to C, angles equal to M L N; this however would lead us to make the middle blocks of the form of inverted wedges (Fig. 20), or at least to make their divisions parallel: but it will be sufficient in practice to cause the parts next the abutments to converge to points somewhat nearer than the point of convergence of the middle parts (Fig. 21); nor, indeed, has this arrangement any material advantage over the simpler form of lines converging to a single centre.

Fig. 20.

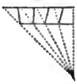


Fig. 21.

From a consideration of these principles, we may derive some useful inferences with respect to arches in general, especially such as are employed in buildings. The objects to be attained in the construction of an arch are to diminish as much as possible the horizontal thrust, and to secure the stability by such an arrangement as requires the least size in the blocks and firmness in the joints. The size of the blocks must be such, that the curve of equilibrium, under the pressure actually produced by the walls, may be everywhere included within their substance, and even without coming very near their termination; and the horizontal thrust will be less in proportion as the curvature at the vertex is greater, that is, other things being equal, as the arch is higher. Supposing the height of the wall supported by the arch to be very considerable in proportion to that of the arch itself, the curve of equilibrium must be very nearly a parabola: if the wall is raised but little above the arch, it will approach to a segment of a circle. In order therefore to find whether the size of the blocks

is sufficient, describe a parabola through the summit and the abutments; and if it pass wholly within the blocks, they will stand; provided however that their joints are either perpendicular to the curve, or are within the limits of the angle of repose on either side of the perpendicular. But if the wall is very low, and the arch flat, a segment of a circle will be more correct than a parabola. Hence it is obvious, first, that a segment of a circle is a better form for an arch than an ellipsis of equal height and span, although less pleasing to the eye, the horizontal thrust being less: secondly, that for the same reason, a Gothic or pointed arch is preferable to a Saxon or semicircular arch, when its height is greater; and even when the height is equal, an arch composed of two parabolic segments meeting in the vertex is stronger than a semicircular arch: for, supposing the wall very high, the depth of the arch stones of a semicircular arch must be at least $\frac{1}{13}$ of the span, in order that the arch may stand, but that of the stones of a Gothic arch, composed of two parabolic segments, may be less by one-twentieth; the parabola of equilibrium touching in this case the internal limit of the arch at $\frac{28}{100}$ of its whole height above the abutments. If, however, the arch is flatter, a segment of a circle will be somewhat stronger than a pointed arch composed of parabolic or elliptical segments. When the arch is higher, it is obvious that a single circular curve is no longer applicable: and in this case, it is of little consequence whether the segments be circular or parabolic, either of these forms approaching sufficiently near to the curve of equilibrium, and both producing equally a much smaller horizontal thrust than a semicircular arch.

No. LI.

REMARKS ON

THE FRICTION OF WHEELWORK,

AND ON THE FORMS BEST SUITED FOR TEETH.

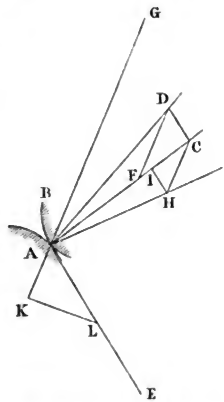
From Buchanan's 'Essay on Wheelwork,' 8vo. Glasgow. 1809.

In a Letter from Dr. Young to the Author.

I have been considering your observations on the difference of the friction, accordingly as the teeth touch before or after the line of centres: at first I was disposed to doubt of the fact; but upon more mature examination, I found that, like many other practical observations, they went beyond the scope of the doctrines of theoretical writers. I cannot, however, perfectly agree with you as to the explanation of the fact: but I will state to you briefly my opinion on the subject, not having leisure at present to enter into a more ample discussion.

The magnitude of the friction has usually been estimated by the relative velocity of the surfaces concerned; a mode of calculation which, as I have observed in my Lecture on Machinery, is only so far correct as it shows the comparative effect of a given friction in retarding the machine. But in fact the primitive friction itself is liable to variation, according to the obliquity of the surfaces: for since the friction is nearly proportional to the mutual pressure, it will be greater or less as the direction of these surfaces is more or less inclined to the radii, the force of rotation being supposed to be given: and what is of still more immediate importance to the resolution of the difficulty in question, the direction of the force by which the one wheel acts on the other, is not to be considered as perpendicular to the surface of the teeth, but as oblique to it, being so situated as to oppose the joint result of the direct resistance and the friction; that is, as being inclined to the surface in a

certain constant angle, which a late anonymous writer* has called the angle of repose, and which is equal to the inclination of a plane, on which one of the substances concerned would begin to slide on the other by its gravitation.



Let the tooth A impel the tooth B with the given force AC, perpendicular to the common surface of the teeth; make C A D equal to the angle of repose, then the force must act in the direction AD, and making C D parallel to the radius A E, A D will be the actual pressure: and then drawing D F parallel to the radius A G, A F will be the effective force in the direction A C, and F C will be the loss by friction. Again, if B impel A, the angle of repose must lie on the other side of A C, and C H must be parallel to A G, and H I to A E, and the friction in this case will be I C, which is obviously less than F C.

Hence we may easily calculate the magnitude of the resistance F C or I C produced by friction, calling the force A C unity; for C D becomes $\frac{\sin.CAD}{\sin.ADC}$, and $FC = CD \cdot \frac{\sin.CDF}{\sin.CFD} = \frac{\sin.CAD}{\sin.ADC} \cdot \frac{\sin.CDF}{\sin.CFD} = \frac{\sin.CAD}{\sin.(CAE+CAD)} \cdot \frac{\sin.GAE}{\sin.GAC}$; and in the same manner $IC = \frac{\sin.CAD}{\sin.AHC} \cdot \frac{\sin.CHI}{\sin.CIH} = \frac{\sin.CAD}{\sin.(GAC+CAD)} \cdot \frac{\sin.GAE}{\sin.EAC}$. Both these quantities vary ultimately as the angle formed by the radii, and vanish when the point of contact is in the line of the centres; and in this case the common theory agrees with this calculation. When C A E is always a right angle, as in the epicycloidal tooth commonly recommended, the friction F C varies in the different positions of the teeth as $\frac{\sin.GAE}{\sin.GAC}$, or as cot. G A C: that is, if A K be made constant, as K L.

* See the last article, No. L., p. 182.

Since therefore it is demonstrable, that the friction is always greater in approaching the line of the centres than at an equal distance beyond it, it must obviously be desirable that the contact should be rather after than before the passage of the teeth over that line, although it is better that it should be at a small distance before, than at a much greater distance beyond it. Hence, the impelling teeth ought to be of such a form as to accelerate the motion of the impelled a little before and a little more after the passage of the line of the centres, and then to retard it again, so that the next tooth may succeed to a similar operation.

A wheel acting on a trundle with cylindrical staves has in this respect an advantage over two wheels with teeth, since the curve fitted for impelling the trundle is adapted only to act on it beyond the line of the centres. This curve may however be formed more easily, and at the same time more advantageously, than by the method which has hitherto been recommended: for if we employ an epicycloid described by the rolling of a circle, which would just touch the internal surface of all the staves of the trundle, on the circumference of the wheel, the trundle will at first be accelerated a very little, and will then be allowed to fall back from each tooth to the succeeding one, soon after its passage over the line of the centres. The same form will also answer very well when the trundle is to impel the wheel, although this mode of action produces a greater friction than the former.

A similar advantage may be obtained in teeth of any other form, by finishing them in such a manner as to project a very little beyond the regular outline, at the point which is intended to come into contact a little beyond the line of the centres. Such a corrected outline may be described at once, if it be required. If the tooth is to be formed into an involute of a circle, having fitted a thread or fine wire to the circumference of the wheel, find the point of contact at the instant when the end of the wire is describing the part of the tooth which is to act at or a little before the line of the centres; cut off from the wheel, beyond this point, an arc equal to the distance of the centres of two adjoining teeth, and fix a pin in the tan-

gent at the same point, that is, in the continuation of the part of the wire which is unrolled, at such a distance as just to stretch the part which is left loose by the removal of the arc; the pin thus fixed, and the remainder of the circle, will serve as bases for continuing the evolution of the wire and the description of the tooth. The same position of the wire will show the outline of a basis proper for describing, by means of a circle rolled on it, the curve which must be substituted for the form of any epicycloidal tooth, which might have been described by causing the same circle to roll on the simple circumference of the wheel as a basis: the curved part of the tooth beginning in this case at the point of contact first mentioned.

The advantage of dividing the pressure among several teeth ought not to be purchased at the expense of an increase of friction, since the property of greater durability may be obtained in an equal degree by simply making the wheels thicker, without materially adding to the friction; and in fact although the pressure on each tooth may be lessened by dividing, yet its duration is increased momentarily in the same proportion.

It has been remarked that the form of the involute of a circle is not immediately deducible from the general principle of Camus; and the remark is strictly true, since the curves formed, according to that principle, from two contiguous circles as bases, would not act on each other without a further separation of the centres, which would render the demonstration inadequate. But I have observed in the additions to my second volume (Lectures, p. x.), that the principle may be extended to any other curves, as well as circles and straight lines: and if we employ an equi-angular spiral instead of a straight line, we shall have the involutes exactly as they are recommended for practice.

No. LII.

SELECTIONS FROM THE ARTICLE

'BRIDGE,'

In the Supplement to the Encyclopædia Britannica.

WRITTEN IN 1816.

THE mathematical theory of the structure of bridges has been a favourite subject with mechanical philosophers; it gives scope to some of the most refined and elegant applications of science to practical utility; and at the same time that its progressive improvement exhibits an example of the very slow steps by which speculation has sometimes followed execution, it enables us to look forwards with perfect confidence to that more desirable state of human knowledge, in which the calculations of the mathematician are authorised to direct the operations of the artificer with security, instead of watching with servility the progress of his labours.

Of the origin of the art of building bridges a sketch has been given in the body of the *Encyclopædia;* the subject has been re-discussed within the last twenty years by some of the most learned antiquaries, and of the most elegant scholars of the age; but additions still more important have been made to the scientific and practical principles on which that art depends; and the principal information, that is demanded on the present occasion, will be comprehended under the two heads of physico-mathematical principles subservient to the theory of this department of architecture, and a historical account of the works either actually executed or projected, which appear to be the most deserving of notice. The first head will contain three sections, relating respectively (1) to the resistance of the materials employed, (2) to the equilibrium of arches, and (3) to the effects of friction; the second will comprehend (4) some details of

earlier history and literature, (5) an account of the discussions which have taken place respecting the improvement of the port of London, and (6) a description of some of the most remarkable bridges which have been erected in modern times.*

Section I.—*Of the Resistance of Materials.*

The nature of the forces on which the utility of the substances employed in architecture and carpentry depends, has been pretty fully investigated in the article Strength of the *Encyclopædia*;† and the theory has been carried somewhat further, in the investigations of a late writer‡ concerning Cohesion and Passive Strength of Materials. Much, however, still remains to be done; and we shall find many cases, in which the principles of these calculations admit of a more immediate and accurate application to practice than has hitherto been supposed. It will first be necessary to advert to the foundation of the theory in its simplest form, as depending on the attractive and repulsive powers, which balance each other, in all natural substances remaining in a permanent state of cohesion, whether as liquids, or as more or less perfect solids.

A.—*In all homogeneous solid bodies, the resistances to extension and compression must be initially equal, and proportional to the change of dimensions.*

The equilibrium of the particles of any body remaining at rest, depends on the equality of opposite forces, varying according to certain laws; and that these laws are continued without any abrupt change, when any minute alteration takes place in the distance, is demonstrated by their continuing little altered by any variation of dimensions, in consequence of an increase or diminution of temperature, and might indeed be at once inferred as highly probable, from the general principle of continuity observed in the laws of nature. We may, therefore, always assume a change of dimensions so small, that, as in all other differential calculations, the elements of the curves, of which the ordinates express the forces, as functions of, or as

* The last of these six sections has not been reprinted.—*Note by the Editor.*
† Written by Professor Robison. ‡ Dr. Young.

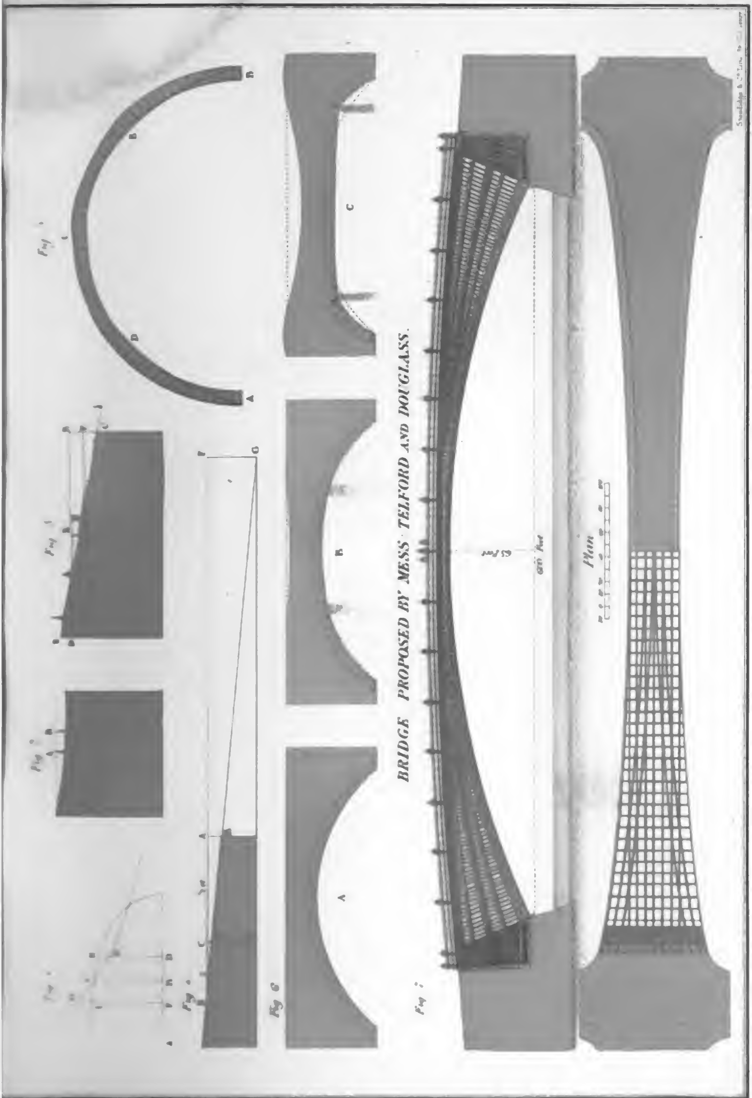
depending on, the distances as abscisses, may be considered as not sensibly differing from right lines crossing each other, if the curves be drawn on the same side of the absciss, in a point corresponding to the point of rest, or to the distance affording an equilibrium; so that the elementary finite differences of the respective pairs of ordinates, which must form, with the portions of the two curves, rectilinear triangles, always similar to each other, will always vary as the lengths of the elements of the curves, or as the elements of the absciss, beginning at the point of rest; and it is obvious that these differences will represent the actual magnitude of the resistances exhibited by the substance to extension or compression. (Plate, fig. 1.)

It was on the same principle that Bernoulli long ago observed, that the minute oscillations of any system of bodies, whatever the laws of the forces governing them might be, must ultimately be isochronous, notwithstanding any imaginable variation of their comparative extent, the forces tending to bring them back to the quiescent position being always proportional to the displacements; and so far as the doctrine has been investigated by experiments, its general truth has been amply confirmed; the slight deviations from the exact proportion, which have been discovered in some substances, being far too unimportant to constitute an exception, and merely tending to show that these substances cannot have been perfectly homogeneous, in the sense here attributed to the word. When the compression or extension is considerable, there may indeed be a sensible deviation, especially in fibrous or stratified substances; but this irregularity by no means affects the admissibility of any of the conclusions which will be derived from this proposition.

B.—*The strength of a block or beam must be reduced to one half, before its cohesive and repulsive forces can both be called into action.*

We must suppose the transverse sections of the body to remain plane and perpendicular to the axis, whatever the point may be to which the force is applied, a supposition which will be correctly true, if the pressure be made by the intervention of a firm plate attached to each end, and which is perfectly admis-

STRUCTURE OF BRIDGES



sible in every other case. Now if the terminal plates remain parallel, it is obvious that the compression or extension must be uniformly distributed throughout the substance, which must happen when the original force is applied in the middle of the block; the centre of pressure or resistance, collected by the plate, acting like a lever, being then coincident with the axis. But when the plates are inclined, the resistance depending on the compression or extension will be various in different parts, and will always be proportional to the distance from the neutral point, where the compression ends and the extension begins, if the depth of the substance is sufficient to extend to this point; consequently the forces may always be represented, like the pressure of a fluid, at different depths, by the ordinates of a triangle; and their result may be considered as concentrated in the centre of gravity of the triangle, or of such of its portions as are contained within the depth of the substance; and when both extension and compression are concerned, the smaller force may be considered as a negative pressure, to be subtracted from the greater, as is usual when any other compound forces are supposed to act on a lever of any kind. Now, when the neutral point is situated in one of the surfaces of the block, the sum of all the forces is represented by the area of the triangle, as it is by that of the parallelogram when the plates remain parallel, and these areas being in either case equivalent to the same external force, it is obvious that the perpendicular of the triangle must be equal to twice the height of the parallelogram, indicating that the compression or extension of the surface in the one case is twice as great as the equable compression or extension in the other; and since there is always a certain degree of compression or extension which must be precisely sufficient to crush or tear that part of the substance which is immediately exposed to it, and since the whole substance must in general give way when any of its parts fail, it follows that the strength is only half as great in the former case as in the latter. And the centre of gravity of every triangle being at the distance of one-third of its height from the base, the external force must be applied in order to produce such a compression or extension, at the distance of one sixth of the depth from the axis; and when

its distance is greater than this, both the repulsive and cohesive forces of the substance must be called into action, and the strength must be still further impaired. (Plate, fig. 2.)

C.—*The compression or extension of the axis of a block or beam is always proportional to the force, reduced to the direction of the axis, at whatever distance it may be applied.*

We may suppose one of the inflexible plates, attached to the extremities of the block, to be continued to the given distance, and to act as a lever held in equilibrium by three forces, that is, by the cohesive and repulsive resistances of the block, and the external force; and it is obvious that, as in all other levers, the external force will always be equal to the difference of the other two forces depending on the compression and extension, or to the mean compression or extension of the whole, which must also be the immediate compression or extension of the middle, since the figure representing the forces is rectilinear. And the effect will be the same, whatever may be the intermediate substances by which the force is impressed on the block, whether continued in a straight line or otherwise. When the force is oblique, the portion perpendicular to the axis will be resisted by the lateral adhesion of the different strata of the block, the compression or extension being only determined by the portion parallel to the axis; and when it is transverse, the length of the axis will remain unaltered. But the line of direction of the original force must always be continued till it meets the transverse section at any point of the length, in order to determine the nature of the strain at that point.

D.—*The distance of the neutral point from the axis of a block or beam is to the depth, as the depth to twelve times the distance of the force, measured in the transverse section.*

Calling the depth a, and the distance of the neutral point from the axis z, the resistances may be expressed by the squares of $\frac{1}{2}a + z$ and $\frac{1}{2}a - z$, which are the sides of the similar triangles denoting the compression and extension (Prop. B.); consequently, the difference of these squares, $2az$, will represent the external force (Prop. C.). But the distance of the

centres of gravity of the two triangles must always be $\frac{2}{3}a$; and, by the property of the lever, making the centre of action of the greater resistance the fulcrum, as the external force is to the smaller resistance, so is this distance to the distance of the force from the centre of action of the greater resistance; or $2az : (\frac{1}{2}a + z)^2 = \frac{2}{3}a : \left(\frac{aa}{12z} - \frac{a}{3} + \frac{z}{3}\right)$; and adding to this the distance of the centre of action from the axis, which must be $\frac{1}{2}a - \frac{1}{3}(\frac{1}{2}a + z) = \frac{1}{3}a - \frac{1}{3}z$, we have $\frac{aa}{12z}$ for the distance of the force from the axis; whence, calling this distance y, $z = \frac{aa}{12y}$.

E.—*The power of a given force to crush a block, is increased, by its removal from the axis, supposing its direction unaltered, in the same proportion as the depth of the block is increased by the addition of six times the distance of the point of application of the force, measured in the transverse section.*

Since the compression or extension of the axis is invariable, whatever the distance of the force may be, that of the nearest surface must be as much greater, by the properties of similar triangles, as the half depth, increased by the distance of the neutral point, is greater than that distance itself, that is, in the ratio of $a + 6y$ to a, since z is to a as a to $12y$; (Prop. D.) and to $\frac{1}{2}a$ as a to $6y$: and the strength is reduced in the same proportion, as the partial compression or extension, by the operation of a given force, is increased. (Plate, fig. 3.)

F.—*The curvature of the neutral line of a beam at any point, produced by a given force, is proportional to the distance of the line of direction of the force from the given point of the axis, whatever that direction may be.*

Since the distance z of the neutral point from the axis is inversely as y, the distance of the force, and the radius of curvature, or the distance of the intersection of the planes of the terminal plates from the neutral point, must be to the distance z as the whole length of the axis is to the alteration of that length produced by the compression or extension, it follows that the radius of curvature must be inversely as the

distance y, and inversely also as the compression, and the curvature itself must be conjointly as the force and as the distance of its application. If the direction of the force be changed, and the perpendicular falling from the given point of the axis on the line of the force be now called y, the distance of the force from the axis measured in the transverse section will be increased by the obliquity exactly in the same ratio as its efficacy is diminished, and the curvature of the neutral line will remain unaltered; although the place of that line will be a little varied, until at last it coincides with the axis, when the force becomes completely transverse; and the radius of curvature of the axis will always be to that of the neutral line as the acquired to the original length of the axis. (Plate, fig. 4.)

G.—*The radius of curvature of the neutral line is to the distance of the neutral point as the original length of the axis to the alteration of that length; or as a certain given quantity to the external force: and this quantity has been termed the Modulus of elasticity.*

Or $r : z = \mathrm{M} : f$, and $r = \frac{\mathrm{M}z}{f} = \frac{\mathrm{M}aa}{12fy}$, as is obvious from the preceding demonstration; y being the distance of the line of the force from the given point, whatever its direction may be.

H.—*The flexibility, referred to the direction of the force, is expressed by unity, increased by twelve times the square of the distance, divided by that of the depth.*

Making the alteration of the axis unity, the corresponding change at the distance y will be to 1 as $z + y$ to z, or as $1 + \frac{y}{z}$ to 1, and will consequently be equal to $1 + \frac{12yy}{aa}$. (Prop. D.)

When the direction of the force becomes oblique, the actual compression of the axis is diminished, but its effect referred to that direction remains unaltered.

I.—*The total compression of a narrow block, pressed in the direction of one of its diagonals, is twice as great as if the same force were applied in the direction of the axis.*

This proposition affords a simple illustration of the application of the preceding one. Calling the length of any portion of the axis x, beginning from the middle, and neglecting the obliquity, the distance of the force may be called $y = nx$, and the compression in the line of the force being everywhere as $1 + \frac{12yy}{aa}$, its fluxion will be $\mathrm{d}x + \mathrm{d}x \frac{12nnxx}{aa}$, and the fluent $x + \frac{4n^2x^3}{aa}$, which, when $y = \frac{1}{2} a$, becomes $x + x$, which is twice as great as if y were always $= 0$. But if the breadth of the block were considerable, so that it approached to a cube, the compression would vary according to a different law, each section parallel to the diagonal affording an equal resistance, and the exact solution of the problem would require an infinite series for expressing the value of $\int n^{\frac{1}{2}} \mathrm{d}x$.

K.—*If a solid bar have its axis curved a little into a circular form, and an external force be then applied in the direction of the chord, while the extremities retain their angular position, the greatest compression or extension of the substance will ultimately be to the mean compression or extension which takes place in the direction of the chord as* $1 + \frac{4h}{a}$ *to* $1 + \frac{16hh}{15aa}$; *a being the depth of the bar, and h the actual versed sine, or the height of the arch.*

We must here separate the actions of the forces retaining the ends of the bar into two parts, the one simply urging the bar in the direction of the chord, and the other, which is of a more complicated nature, keeping the angular direction unaltered; and we must first calculate the variation of the angular situation of the ends, in consequence of the bending of the bar by the first portion, and then the strain required to obviate that change, by means of a force acting in the direction of the middle of the bar, while the ends are supposed to be fixed. If each half of the bar were rectilinear, these two strains would obviously be equal, and would neutralize each other in the middle of the halves, which might be considered as the meeting of the ends of two shorter pieces, acting transversely or obliquely on each other, without any strain; the curvature produced by the whole

strain being elsewhere as the distance from the line joining these points. But, since the bar is supposed to be curved, it becomes necessary to determine the place of these neutral points, by calculating the change of its angular position throughout its extent.

Considering, first, the middle of the bar as fixed, and calling the angular extent of the variable arc x, beginning from the middle, and the radius r, the ordinate y, or the distance of the arc from the chord, will be $r \cos. x - b$, b being the cosine of the whole arc; and the fluxion of the change of the angular situation, being as the strain and the fluxion of the arc conjointly, will be expressed by $pr \cos. x\mathrm{d}x - pb\mathrm{d}x$, of which the fluent is $pr \sin. x - pbx$. In the second place, the curvature derived from the force acting between the two halves, when the ends are considered as fixed points, will be as $r - r\cos. x$, and the fluent of the change of angular situation may be called $qrx - qr \sin. x$; and at the end, when x becomes equal to c, the whole extent of the arc, these two deviations must destroy each other, since the positions of the middle and of the ends remain unaltered; consequently $pr\sin. c - pbc = qrc - qr\sin. c$, whence $\frac{p}{q} = \frac{rc - r\sin. c}{r\sin. c - bc}$, and the exact proportion of p to q may be found, by means of a table of sines. But when the arc is small, $\sin. c$ being equal to $c - \frac{1}{6} c^3 + \frac{1}{120} c^5 \ldots$, $rc - r\sin. c$ is $\frac{1}{6} rc^3$, and $r\sin. c - bc = (r - b)\, c - \frac{1}{6} rc^3$; now $r - b$, the versed sine of the arc, becomes ultimately $\frac{1}{2} rc^2$, and $(r - b)\, c = \frac{1}{2} rc^3$: therefore $p : q = \frac{1}{6} : \frac{1}{3} = \frac{1}{2} : 1$; that is, the strain at the middle, expressed by p, must be half as great as the strain at the ends, expressed by q: consequently, when the force is considered as single, the distance d of the line of its direction from the summit must ultimately be one-third of the versed sine or height.

Now if we call any portion of the chord x, we have for the corresponding value of y, the distance from the line of direction of this force, $\surd\,(r^2 - x^2) - d$, and for the fluxion of the compression or extension in the direction of the chord, $\mathrm{d}x \left(1 + \frac{12yy}{aa}\right)$, which will be true for both portions of the bar, whether y be

positive or negative; but $y^2 = r^2 - x^2 + d^2 - 2d\sqrt{(r^2 - x^2)}$, and the fluent becomes $x + \frac{12}{aa}\left(r^2 x - \frac{1}{3}x^3 + d^2 x - 2d\left[r^2 \text{ arc sin. } \frac{x}{r} - x\sqrt{(r^2 - x^2)}\right]\right)$. When the arc is small, calling the whole versed sine h, we have $y = \frac{1}{3}h - \frac{xx}{2r}$, and $y^2 = \frac{1}{9}h^2 - \frac{hx^2}{3r} + \frac{x^4}{4r^2}$, and the fluent is $x + \frac{12}{aa}\left(\frac{1}{9}h^2 x - \frac{hx^3}{9r} + \frac{x^5}{20r^2}\right)$; but when x becomes equal to the semichord c, h being $\frac{cc}{2r}$, the expression becomes $c + \frac{12}{aa}\left(\frac{c^5}{36r^2} - \frac{c^5}{18r^2} + \frac{c^5}{20r^2}\right) = c + \frac{4c^5}{15a^2r^2} = c + \frac{16h^2c}{15a^2}$, which shows the compression or extension in the line of the chord, while c expresses that which the bar would have undergone if it had been straight, and the force had been immediately applied to the axis; the actual change being greater in the proportion of $1 + \frac{16hh}{15aa}$ to 1.

The greatest strain will obviously be at the ends, where the distance from the line of direction of the force is the greatest, the compression or extension of the surface being here to that of the axis, as $a + 6y$ to a (Prop. E.) or as $1 + \frac{4h}{a}$ to 1; consequently the compression or extension in the line of the chord is to the greatest actual change of the substance as $1 + \frac{16hh}{15aa}$ to $1 + \frac{4h}{a}$.

Thus if the depth a were 10 feet, and the height or versed sine $h = 20$, the radius being very large, the whole compression of the chord would be to the whole compression of a similar substance, placed in the direction of the chord, as 5.267 to 1; and the compression at the surface of the ends would be to the compression of the axis there as 9 to 1; and disregarding the insensible obliquity, this compression may be considered as equal throughout the bar; so that the compression at the ends will be to the compression of the chord as 9 to 5.267, or as 17 to 10.

Supposing, for example, such a bar of iron to undergo a change of temperature of 32° of Fahrenheit, which would natu-

rally cause it to expand or contract about $\frac{1}{3000}$ in all its dimensions; then the length of the chord, being limited by the abutments, must now be supposed to be altered $\frac{1}{3000}$ by an external force; and, at the extremities of the abutments, the compression and extension of the metal will amount to about $\frac{1}{3000}$; a change which is equivalent to the pressure of a column of the metal about 3300 feet in height, since M, the height of the modulus of elasticity, is found, for iron and steel, to be about 10,000,000 feet; and such would be the addition to the pressure at one extremity of the abutment, and its diminution at the other, amounting to about five tons for every square inch of the section, which would certainly require some particular precaution, to prevent the destruction of the stones forming the abutment by a force so much greater than they are capable of withstanding without assistance. Should such a case indeed actually occur, it is probable that the extremities would give way a little, and that the principal pressure would necessarily be supported nearer the middle, so that there would be a waste of materials in a situation where they could co-operate but imperfectly in resisting the thrust; an inconvenience which would not occur if the bar were made wider and less deep, especially towards the abutments.

Section II.—*Of the Equilibrium of Arches.*

We may now proceed to inquire into the mode of determining the situation and properties of the curve of equilibrium, which represents, for every part of a system of bodies supporting each other, the general direction of their mutual pressure; remembering always that this curve is as much an imaginary line, as the centre of gravity is an imaginary point, the forces being no more actually collected into such a line than the whole weight or inertia of a body is collected in its centre of gravity. Indeed, the situation of the curve is even less definite than that of the centre of gravity, since in many cases it may differ a little according to the nature of the co-operation of the forces which it is supposed to represent. In reality, every gravitating atom entering the structure must be supported by some forces continued in some line, whether regular or irregular, to the fixed

points or abutments, and every resisting atom partakes, in a mathematical sense, either positively or negatively, in transmitting a lateral pressure where it is required for supporting any part of the weight: and when we attempt to represent the result of all these collateral pressures by a simple curve, its situation is liable to a slight variation, according to the direction in which we suppose the co-operating forces to be collected. If, for instance, we wished to determine the stability of a joint, formed in a given direction, it would be necessary to consider the magnitude of the forces acting throughout the extent of the joint in a direction perpendicular to its plane, and to collect them into a single result, and it is obvious that the forces, represented by the various elementary curves, may vary very sensibly in their proportion, when we consider their joint operation on a vertical or on an oblique plane; although if the depth of the substance be inconsiderable, this difference will be wholly imperceptible, and in practice it may generally be neglected without inconvenience; calculating the curve upon the supposition of a series of joints in a vertical direction. If, however, we wish to be very minutely accurate, we must attend to the actual direction of the joints in the determination of the curve, and must consider, in the case of a bridge, the whole weight of the structure terminated by a given arch stone, with the materials which it supports, as determining the direction of the curve of equilibrium where it meets the given joint, instead of the weight of the materials terminated by a vertical plane passing through the point of the curve in question, which may sometimes be very sensibly less; this consideration being as necessary for determining the circumstances under which the joints will open, as for the more imaginary possibility of the arch stones sliding upwards or downwards. But we may commonly make a sufficiently accurate compensation for this difference, by supposing the specific gravity of the materials producing the pressure, and the curvature of the line which terminates them, to be a little increased, while the absciss remains equal to that of the curve of equilibrium intersecting the joints.

L.—*If two equal parallelepipeds be supported each at one end, and lean against each other at the other, so as to remain horizontal, the curve of equilibrium, representing the general effect of the pressure transmitted through them, will be of a parabolic form.*

The pressure of the blocks where they meet, will obviously be horizontal, but at the other ends it will be oblique, being the result of this horizontal pressure and of the whole weight of each block. And if we imagine the blocks to be divided into any number of parts, by sections parallel to the ends, which is the only way in which we can easily obtain a regular result, it is evident that the force exerted at any of these sections, by the external portions, must be sufficient to support the lateral thrust and the weight of the internal portions; and its inclination must be such that the horizontal base of the triangle of forces must be to the vertical perpendicular as the lateral thrust to the weight of the internal portion; or, in other words, the lateral thrust remaining constant, the weight supported will be as the tangent of the inclination. But calling the horizontal absciss x, and the vertical ordinate y, the tangent of the inclination will be $\frac{dy}{dx}$; which, in the case of a parallelepiped, must be proportional to the distance x from the contiguous ends; and $x = \frac{mdy}{dx}$; consequently $xdx = mdy$, and $\frac{1}{2}x^2 = my$, which is the equation of a parabola. It is usual in such cases to consider the thrusts as rectilinear throughout, and as meeting in the vertical line passing through the centre of gravity of each block; but this mode of representation is evidently only a convenient compendium.

If the blocks were united together in the middle, so as to form a single bar or lever, the forces would be somewhat differently arranged; the upper half of the bar would contain a series of elementary arches, abutting on a series of similar elementary chains in the lower half, so as to take off all lateral thrust from the supports at the ends.

With respect to the transverse strains of levers in general, it may be observed, that the most convenient way of representing them is to consider the axis of the lever as composed of a series

of elementary bars, bisected, and crossed at right angles, by as many others extending across the lever, or rather as far as two-thirds of the half depth on each side, where the centre of resistance is situated. The transverse force must then be transmitted unaltered throughout the whole system, acting in contrary directions at the opposite ends of each of the elementary bars constituting the axis; and it must be held in equilibrium, with respect to each of the centres, considered as a fulcrum, by the general result of all the corpuscular forces acting on the longer cross arms; that is, by the difference of the compression or extension on the different sides of the arms. This difference must therefore be constant; and in all such cases the strain or curvature must increase uniformly, and its fluxion must be constant; but if the transverse force be variable, as when the lever supports its own weight, or any further external pressure, the fluxion of the curvature must be proportional to it. Now the transverse force, thus estimated, being the sum of the weights or other forces acting on either side of the given point, the additional weight at the point will be represented by the fluxion of the weight, or by the second fluxion of the strain or curvature, which is ultimately as the fourth fluxion of the ordinate. Also, the fluxion of the strain being as the whole weight on each side, it follows that when the strain is a maximum, and its fluxion vanishes, the whole weight or the sum of the positive and negative forces on either side, must also vanish; as Mr. Dupin has lately demonstrated in a different manner.

M.—*In every structure supported by abutments, the tangent of the inclination of the curve of equilibrium to the horizon is proportional to the weight of the parts interposed between the given point and the middle of the structure.*

The truth of this proposition depends on the equality of the horizontal thrust throughout the structure, from which it may be immediately inferred, as in the last proposition. The materials employed for making bridges are not uncommonly such, as to create a certain degree of lateral pressure on the outside of the arch; but as there must be a similar and equal pressure in a contrary direction against the abutment, its effects will be

comprehended in the determination of the point at which the curve springs from the abutment, as well as in the direction of the curve itself: so that the circumstance does not afford any exception to the general truth of the law. It is, however, seldom necessary to include the operation of such materials in our calculations, since their lateral pressure has little or no effect at the upper part of the arch, which has the greatest influence on the direction of the curve; and it is also desirable to avoid the unnecessary employment of these soft materials, because they tend to increase the horizontal thrust, and to raise it to a greater height above the foundation of the abutment.

We have therefore generally $\int w\,dx = mt = m\,\frac{dy}{dx}$, w being the height of uniform matter, pressing on the arch at the horizontal distance x from the vertex, t the tangent of the inclination of the curve of equilibrium, y its vertical ordinate, and m a quantity proportional to the lateral pressure, or horizontal thrust.

N.—*The radius of curvature of the curve of equilibrium is inversely as the load on each part, and directly as the cube of the secant of the angle of inclination to the horizon.*

The general expression for the radius of curvature is $r = \frac{(dz)^3}{dx\,ddy}$; and here, since $m\,dy = dx\int w\,dx$, dx being constant, $m\,d^2y = w\,(dx)^2$; but dz being $= dx\,\sqrt{(1 + t^2)}$, $\frac{(dz)^2}{ddy} = \frac{m}{w}\,(1 + t^2)$ and $r = \frac{m}{w}\,(1 + t^2)^{\frac{3}{2}}$; and m being constant, r is inversely as the load w, and directly as the cube of the secant $\sqrt{(1 + t^2)}$. The same result may also be obtained from a geometrical consideration of the magnitude of the versed sine of the elementary arc, and the effect of the obliquity of the pressure; the one varying as the square of the secant, the other as the secant simply.

O.—*Consequently, if the curve be circular, the load must be everywhere as the cube of the secant.*

P.—*If the curve of equilibrium be parabolic, the load must be uniform throughout the span.*

(Prop. L.) The uniformity of the load implies that the superior and inferior terminations of the arch, commonly called the extrados and intrados, should be parallel: but it is not necessary that either of them should be parabolic, unless we wish to keep the curve exactly in the middle of the whole structure. When the height of the load is very great in proportion to that of the arch, the curve must always be nearly parabolic, because the form of the extrados has but little comparative effect on the load at each point.

A parabola will therefore express the general form of the curve of equilibrium in the flat bands of brick or stone, commonly placed over windows and doors, which, notwithstanding their external form, may very properly be denominated flat arches. But if we consider the direction of the joints as perpendicular to the curve, it may easily be shown, from the properties of the wedge, that they must tend to a common axis, in order that the thrust may be equal throughout; and the curve must be perpendicular to them, and consequently circular; but the difference from the parabola will be wholly inconsiderable.

Q.—*For a horizontal extrados, and an intrados terminated by the curve itself, which, however, is a supposition merely theoretical, the equation of the curve is* $x = \sqrt{m}\,\text{HL}\,\dfrac{y+\sqrt{(yy-aa)}}{a}$.

Since in this case $w = y$ (Prop. M.), we have $\int y\,\mathrm{d}x = m\,\dfrac{\mathrm{d}y}{\mathrm{d}x}$; and $m\mathrm{d}^2y = y\,(\mathrm{d}x)^2$; whence, multiplying both sides by $\mathrm{d}y$, we have $m\mathrm{d}y\mathrm{d}^2y = y\mathrm{d}y\,(\mathrm{d}x)^2$; and, taking the fluent, $\frac{1}{2}m\,(\mathrm{d}y)^2 = \frac{1}{2}y^2\,(\mathrm{d}x)^2$, and $mt^2 = y^2$, which must be corrected by making $y = a$ when t vanishes, so that we shall have $mt^2 = y^2 - a^2$, and $y = \sqrt{(a^2 + mt^2)}$. But since $\dfrac{\mathrm{d}y}{\mathrm{d}x} = t = \sqrt{\left(\dfrac{yy-aa}{m}\right)}$, $\mathrm{d}x = \mathrm{d}y\sqrt{\dfrac{m}{(yy-aa)}}$, and $x = \sqrt{m}\,\text{HL}\left(y + \sqrt{[y^2 - a^2]}\right) - \sqrt{m}\,\text{HL}\,a$; whence all the points of the curve may be determined by means of a table of logarithms. But such a calculation is by no means so immediately applicable to practice, as has generally been supposed; for the curve of equilibrium will

always be so distant from the intrados at the abutments, as to derange the whole distribution of the forces concerned.

R.—*For an arch of equable absolute thickness throughout its length, the equation is* $z = \surd\,(y^2 - m^2)$ *and* $x = m\,\text{HL}\,\frac{y + \surd\,(yy - mm)}{m}$.

The weight of any portion of the half arch being represented by its length z, we have $z = m\frac{\mathrm{d}y}{\mathrm{d}x}$; but $\mathrm{d}z = \mathrm{d}y\,\surd\,\left(1 + \left(\frac{\mathrm{d}x}{\mathrm{d}y}\right)^2\right) = \mathrm{d}y\,\surd\,\left(1 + \frac{mm}{zz}\right)$, and $\mathrm{d}y = \frac{\mathrm{d}z}{\surd\left(1 + \frac{mm}{zz}\right)} = \frac{z\mathrm{d}z}{\surd(zz + mm)}$, of which the fluent is $\surd\,(z^2 + m^2)$, requiring no further correction than to suppose y initially equal to m; and we have $z = \surd\,(y^2 - m^2)$. Again, since $\mathrm{d}z = \mathrm{d}x\,\surd\left(1 + \frac{zz}{mm}\right)$ we find in the same manner $\mathrm{d}x = \frac{m\mathrm{d}z}{\surd\,(mm + zz)}$, and $x = m\text{HL}\left(z + \surd[mm + zz]\right) - m\text{HL}m = m\text{HL}\,\frac{z + y}{m}$. This curve will, therefore, in some cases, be identical with that of the preceding proposition. It is commonly called the catenaria, since it represents the form in which a perfectly flexible chain of equable thickness will hang by its gravity.

S.—*If the load on each point of an arch be expressed by the equation* $w = a + bx^2$, *the equation for the curve of equilibrium will be* $my = \frac{1}{2}\,ax^2 + \frac{1}{12}\,bx^4$

Since the whole load $\int w\mathrm{d}x$ is here $ax + \frac{1}{3}\,bx^3$, we have $m\,\frac{\mathrm{d}y}{\mathrm{d}x} = ax + \frac{1}{3}\,bx^3$, (Prop. M.) and $my = \frac{1}{2}\,ax^2 + \frac{1}{12}\,bx^4$.

This expression will, in general, be found sufficiently accurate for calculating the form of the curve of equilibrium in practical cases; and it may easily be made to comprehend the increase of the load from the obliquity of the arch-stones. The ordinate y, at the abutment, being given, the value of m may be deduced from it: and since at the vertex my is simply $\frac{1}{2}ax^2$, the radius of curvature r will here be $\frac{xx}{2y} = \frac{m}{a}$.

T.—*If we divide the span of an arch into four equal parts, and add to the weight of one of the middle parts one-sixth of its difference from the weight of one of the extreme parts, we shall have a reduced weight, which will be to the lateral thrust as the height of the arch to half the span, without sensible error.*

The weight of the half arch being expressed by $ax + \frac{1}{3} bx^3$, when x is equal to the whole span, if we substitute $\frac{1}{2} x$ for x, it will become $\frac{1}{2} ax + \frac{1}{24} bx^3$, for one of the middle parts, leaving $\frac{1}{2} ax + \frac{7}{24} bx^3$, for the extreme part, which gives $\frac{6}{24} bx^3$ for the difference of the parts, and $\frac{1}{6}$ of this, added to the former quantity, makes it $\frac{1}{2} ax + \frac{1}{12} bx^3$: but since $my = \frac{1}{2} ax^2 + \frac{1}{12} bx^4$, dividing by mx, we have $\frac{y}{x} = \frac{\frac{1}{2} ax + \frac{1}{12} bx^3}{m}$.

It is also obvious, that if we subtract, instead of adding, one sixth of the difference, we have $\frac{1}{2} ax$; and dividing by $\frac{1}{2} x$, we obtain a, and thence $r = \frac{m}{a}$. m being previously found by the proposition.

U.—*When the load is terminated by a circular or elliptical arc,* $w = a + nb - n \sqrt{(b^2 - x^2)}$, *and* $my = \frac{1}{2} (a + nb)\, x^2 - \frac{1}{2} nb^2 x$ arc sine $\frac{x}{b} - \frac{1}{2} nb^2 \sqrt{(b^2 - x^2)} + \frac{1}{6} n (b^2 - x^2)^{\frac{3}{2}} + \frac{1}{3} nb^3$.

The whole load $\int w dx$ is here $ax + nbx - \frac{1}{2} nb^2$ arc sine $\frac{x}{b} - \frac{1}{2} nx \sqrt{(b^2 - x^2)}$; and hence $my = \frac{1}{2} ax^2 + \frac{1}{2} nbx^2 - \frac{1}{2} nb^2 x$ arc sine $\frac{x}{b} + \frac{1}{2} nb^3 - \frac{1}{2} nb^2 \sqrt{(b^2 - x^2)} + \frac{1}{6} n (b^2 - x^2)^{\frac{3}{2}} - \frac{1}{6} nb^3$ (Prop. M.) And the radius of curvature at the vertex will again be $\frac{m}{a}$. When the curve is circular, the axes of the ellipsis being equal, $n = 1$.

If the extrados and intrados are concentric, the calculation requires us to take the difference between the results determining the weight for each curve; but it will commonly be equally accurate in such a case, to consider the depth of the load

as uniform, at least when the joints are in the direction of the radii.

X.—*The abutment must be higher without than within, by a distance, which is to its breadth, as the horizontal distance of the centre of gravity of the half arch from the middle of the abutment is to the height of the middle of the key-stone above the same point.*

This proposition follows immediately from the proportion of the horizontal thrust to the weight, determined by the property of the lever: the one acting at the distance of the height of the arch from the fulcrum, and the other at the distance of the centre of gravity from the abutment, so as to balance each other; and the oblique direction of the face of the abutment being perpendicular to the thrust compounded of these two forces. The same rule also serves for determining the proper position of the abutment of a beam or rafter of any kind, in order that it may stand securely, without the assistance of friction. But for a bridge, if we calculate the situation of the curve of equilibrium, we obtain the direction of the thrust at its extremity more conveniently, without immediately determining the place of the centre of gravity.

Y.—*In order that an arch may stand without friction or cohesion, a curve of equilibrium, perpendicular to all the surfaces of the joints, must be capable of being drawn within the substance of the blocks.*

If the pressure on each joint be not exactly perpendicular to the surfaces, it cannot be resisted without friction, and the parts must slide on each other: this, however, is an event that can never be likely to occur in practice. But if the curve, representing the general pressure on any joint, be directed to a point in its plane beyond the limits of the substance, the joint will open at its remoter end, unless it be secured by the cohesion of the cements, and the structure will either wholly fall, or continue to stand in a new form. (Plate, fig. 5.)

From this condition, together with the determination of the

direction of the joints already mentioned (Prop. P.), we may easily find the best arrangement of the joints in a flat arch; the object, in such cases, being to diminish the lateral thrust as much as possible, it is obvious that the common centre of the joints must be brought as near to the arch as is compatible with the condition of the circle remaining within its limits; and it may even happen that the superincumbent materials would prevent the opening of the joints even if the centre were still nearer than this: but if, on the other hand, the arch depended only on its own resistance, and the materials were in any danger of being crushed, it would be necessary to keep the circle at some little distance from its surfaces, even at the expense of somewhat increasing the lateral pressure.

When the curve of equilibrium touches the intrados of an arch of any kind, the compression at the surface must be at least four times as great as if it remained in the middle of the arch-stones (Prop. E.), and still greater than this if the cohesion of the cements is called into action. In this estimate we suppose the transverse sections of the blocks inflexible, so as to co-operate throughout the depth in resisting the pressure on any point; but in reality this co-operation will be confined within much narrower limits, and the diminution of strength will probably be considerably greater than is here supposed, whenever the curve approaches to the intrados of the arch.

The passage of the curve of equilibrium through the middle of each block is all that is necessary to ensure the stability of a bridge of moderate dimensions and of sound materials. Its strength is by no means increased, like that of a frame of carpentry, or of a beam resisting a transverse force, by an increase of its depth in preference to any other of its dimensions: a greater depth does, indeed, give it a power of effectually resisting a greater force of external pressure derived from the presence of any occasional load on any part of the structure; but the magnitude of such a load is seldom very considerable, in proportion to the weight of the bridge.

It is of some importance, in these investigations, to endeavour to trace the successive steps by which the fabric of a bridge may commonly be expected to fail. Supposing the materials to be

too soft, or the abutments insecure, or any part of the work to be defective, and to afford too little resistance, the length of the curve of the arch being diminished, or its chord extended, it will become flatter, and, consequently, sink; the alteration being by far the greatest, if other things are equal, where the depth is the least, that is, near the crown or key-stone; so that if the curvature was, at first, nearly equal throughout, the crown will sink so much as to cause a rapid increase of curvature on each side in its immediate neighbourhood, which will bring the intrados up to the curve of equilibrium, or even above it, the form of this curve being little altered by the change of that of the arch. The middle remains firm, because the pressure is pretty equally divided throughout the blocks, but the parts newly bent give way to the unequal force, and chip a little at their internal surface; but being reduced in their dimensions by the pressure, they suffer the middle to descend still lower, and are, consequently, carried down with it, so as to be relieved from the inequality of pressure depending on their curvature, and to transfer the effect to the parts immediately beyond them, till these in their turn crumble, and by degrees the whole structure falls. (Plate, Fig. 6.)

This explanation will enable us to understand some observations and experiments which the late Professor Robison has related as somewhat paradoxical. He says, that an arch built "of an exceedingly soft and friable stone," the arch-stones being also too short, began to show signs of weakness by the stones chipping about ten feet from the middle, and that it afterwards split at the middle, and fifteen or sixteen feet on each side of it, and also at the abutments. And in some experiments on models of arches in chalk, he found, that "the arch always broke at some place considerably beyond another point, where the first chipping had been observed;" a circumstance which he has not succeeded in sufficiently explaining.

Section III.—*Of the Effect of Friction.*

The friction or adhesion of the substances, employed in Architecture, is of the most material consequence, for insuring

the stability of the works constructed with them; and it is right that we should know the extent of its operation; it is not, however, often practically necessary to calculate its exact magnitude, because it would seldom be prudent to rely materially on it, the accidental circumstances of agitation or moisture tending very much to diminish its effect. Nor is the cohesion of the cements employed of much further consequence than as enabling them to form a firm connexion, by means of which the blocks may rest more completely on each other than they could do without it; for we must always remember, that we must lose at least half of the strength, before the cohesion of the solid blocks themselves, in the direction of the arch, can be called into action, and at least three fourths before the joints will have any tendency to open throughout their extent.

Z.—*The joints of an arch, composed of materials subject to friction, may be situated in any direction lying within the limits of the angle of repose, on either side of the perpendicular to the curve of equilibrium; the angle of repose being equal to the inclination to the horizon at which the materials begin to slide on each other; and the direct friction being to the pressure as the tangent of this angle is to the radius.*

It is obvious, that any other force, as well as that of gravity, will be resisted by the friction or adhesion of the surfaces when its direction is within the limits of the angle at which the substances begin to slide: and it may be inferred from the experiments of Mr. Coulomb and Professor Vince, that this angle is constant, whatever the magnitude of the force may be, since the friction is very nearly proportional to the mutual pressure of the substances. The tendency of a body to descend along any plane being as much less than its weight as the height of the plane is less than its length, and the pressure on the plane being as much less than the weight as the length is greater than the horizontal extent, it follows, that, when the weight begins to overcome the friction, the friction must be to the pressure as the height of the plane to its horizontal extent, or as the tangent of the inclination to the radius.

This property of the angle of repose affords a very easy

method of ascertaining, by a simple experiment, the friction of the materials employed: taking, for example, a common brick, and placing it, with the shorter side of its end downwards, on another which is gradually raised, we shall find that it will fall over without beginning to slide; and when this happens, the height must be half of the horizontal extent, a brick being twice as long as it is broad: in this case, therefore, the friction must be at least half of the pressure, and the angle of repose at least 30°; and an equilateral wedge of brick could not be forced up by any steady pressure of bricks acting against its sides, in a direction parallel to its base. But the effects of agitation would make such a wedge totally insecure in any practical case; and the determination only serves to assure us, that a very considerable latitude may be allowed to the joints of our materials, when there is any reason for deviating from the proper direction, provided that we be assured of a steady pressure; and much more in brick or stone than in wood, and more in wood than in iron, unless the joints of the iron be secured by some cohesive connexion. It may also be inferred from these considerations, that the direction of the joints can never determine the direction of the curve of equilibrium crossing them, since the friction will always enable them to transmit the thrust in a direction varying very considerably from the perpendicular; although, with respect to any particular joint, of which we wish to ascertain the stability independent of the friction, it would be desirable to collect the result of the elements, of which that curve is the representative, with a proper regard to its direction.

Section IV.—*Earlier Historical Details.*

The original invention of arches, and the date of their general adoption in architecture, have been discussed with great animation by the late Mr. King, Mr. Dutens, and several other learned antiquaries. Mr. King insisted that the use of the arch was not more ancient than the Christian era, and considered its introduction as one of the most remarkable events accompanying that memorable period. Mr. Dutens appealed

to the structure of the cloacæ, built by the Tarquins, and to the authority of Seneca, who observes that the arch was generally considered as the invention of Democritus, a Philosopher who lived some centuries before Christ, but that, in his opinion, the simplicity of the principle could not have escaped the rudest architect; and, that long before Democritus, there must have been both bridges and doors, in both of which structures the arch was commonly employed. There do indeed appear to be solitary instances of arches more ancient than the epoch assigned by Mr. King to their invention. We find arches concealed in the walls of some of the oldest temples extant at Athens; the cloacæ are said to be arched, not at the opening into the Tiber only, but to a greater distance within it than is likely to have been rebuilt at a later period for ornament; and the fragments of a bridge, still remaining at Rome, bear an inscription which refers its erection to the latter years of the Commonwealth. But it seems highly probable, that almost all the covered ways, constructed in the earlier times of Greece and Rome, were either formed by lintels, like doorways, or by stones overhanging each other, in horizontal strata, and leaving a triangular aperture, or by both these arrangements combined, as is exemplified in the entrance to the treasury of Atreus at Mycenæ, where the lintel has a triangular aperture over it, by which it is relieved from the pressure of the wall above; and this instance serves to show how different the distribution of the pressure on any part of a structure may be, from the simple proportions of the height of the materials above it. Some other old buildings, which have been supposed to be arched, have been found, on further examination, rather to resemble domes, which may be built without centres, and may be left open at the summit, the horizontal curvature producing a transverse pressure, which supports the structure without an ordinary key-stone. And this has been suspected to be the form of the roofs and ceilings of ancient Babylon, where Strabo tells us that the buildings were arched over or "camerated," for the purpose of saving timber: and the bridge of Babylon, which must have been of considerable antiquity, is expressly said, by Herodotus, to have consisted of piers of stone, with a road formed of beams of wood only. It

may however be rejoined, that though a dome is not simply an arch, yet it exceeds it in contrivance and mechanical complication; it generally exerts a thrust, and requires either an abutment, or a circular tie; and it is scarcely possible that the inventor of a dome should not have been previously acquainted with the construction of a common arch. Besides the term CAMARA, the Greeks had also PSALIS, APSIS, and THOLUS; the last was particularly appropriate to circular domes; but the variety of appellations seems to prove that the thing must have been perfectly familiar; and the term PSALIS is supposed to have been applied from the appearance of the wedged archstones, viewed in their elevation, which could not have been observable in a dome of any kind.

From these outlines of the origin of the art of building bridges, we may pass on rapidly to the latest improvements which have been made, in Great Britain and on the Continent, in the practice of this department of architecture. A very ample detail of the most important operations, that are generally required to be performed in it, may be found in the numerous Reports of the ingenious Mr. Smeaton, published since his death by the Society of Civil Engineers in London. They contain a body of information comprehending almost every case that can occur to a workman, in the execution of such structures; and even where they have to record an accidental failure, the instruction they afford is not less valuable than where the success has been more complete.

Respecting the general arrangement of a bridge, and the number of arches to be employed, in the case of a wide river, Mr. Smeaton has expressed his approbation of a few wide and flat arches, supported by good abutments, in preference to more numerous piers, which unnecessarily interrupt the water-way. In a case where a long series of small arches was required, he has made them so flat, and the piers so slight, that a single pier would be incapable of withstanding the thrust of its arch: but in order to avoid the destruction of the whole fabric in case of an accident, he has intermixed a number of stronger piers, at certain intervals, among the weaker ones. Where several arches, of different heights, were required, he commonly recom-

mended different portions of the same circle for all of them; a mode which rendered the lateral thrust nearly equal throughout the fabric, and had the advantage of allowing the same centre to be employed for all, with some little addition at the ends to adapt it to the larger arches. He records the case of Old Walton Bridge, in which the wooden superstructure had sunk two feet, so as to become part of a circle 700 feet in diameter, and the thrust, thus increased, had forced the piers considerably out of their original situation: a striking proof that the principles of the pressure of arches must not be neglected, even when frames of carpentry are concerned.

Mr. Smeaton particularly describes the inconveniences arising from the old method of laying the foundations of piers, which was introduced soon after the Conquest, and which is particularly exemplified in London Bridge. The masonry commences above low water mark, being supported on piles, which would be exposed to the destructive alternation of moisture and dryness, with the access of air, if they were not defended by other piles, forming projections partly filled with stone, and denominated sterlings; which, in their turn, occasionally require the support and defence of new piles surrounding them, since they are not easily removed when they decay; so that, by degrees, a great interruption is occasioned by the breadth of the piers, thus augmented, requiring, for the transmission of the water, an increase of velocity, which is not only inconvenient to the navigation, but also carries away the bed of the river under the arches, and immediately below the bridge, making deep pools or excavations, which require from time to time to be filled up with rubble stones; while the materials, which have been carried away by the stream, are deposited a little lower down in shoals, and very much interfere with the navigation of the river. From these circumstances, as well as from the effects of time and decay, it has happened, according to late reports, that the repairs of London Bridge have often amounted, for many years together, to 4000*l.* a year, while those of Westminster and Blackfriars Bridges have not cost so many hundreds. It is true, that the fall produces a trifling advantage in enabling the London water-works to employ more of the force of the

tide in raising water for the use of the city; and this right, being established as a legal privilege, has long delayed the improvements, which might otherwise have been attempted, for the benefit of the navigation of the river. The interest of the proprietors of the water-works has been valued at 125,000*l.*; and it has been estimated that 50,000*l.* would be required for the erection of steam-engines to supply their place; while, on the other hand, it is said that from thirty to forty persons, on an average, have perished annually from the dangers of the fall under the bridge.

But Mr. Smeaton, as well as his predecessor Mr. Labelye, appears sometimes to have gone into a contrary extreme, and to have been somewhat too sparing in the use of piles. It is well known that the opening of Westminster Bridge was delayed for two years on account of the failure of a pier, the foundation of which had been partly undermined by the incautious removal of gravel from the bed of the river, in its immediate neighbourhood; a circumstance which would scarcely have occurred if piles had been more freely employed in securing the foundation. The omission, however, did not arise from a want of a just estimate of the importance of piles in a loose bottom, but from a confidence, founded on examination as the work advanced, that the bed of the river was already sufficiently firm. Mr. Smeaton directed the foundations of Hexham Bridge to be laid, as those of Westminster Bridge had been, by means of caissons, or boxes, made water-tight, and containing the bottom of the pier, completed in masonry well connected together, and ready to be deposited in its proper place by lowering the caissons, and then detaching the sides, which are raised for further use, from the bottoms, which remain fixed as a part of the foundations immediately resting on the bed of the river, previously made smooth for their reception, and sometimes also rendered more firm by piles and a grating of timber. By a careful examination of the bottom of the river at Hexham, Mr. Smeaton thought he had ascertained that the stratum of gravel, of which it consisted, was extremely thin, and supported by a quicksand, much too loose to give a firm hold to piles, while he supposed the gravel strong enough to bear the weight of the pier, if built in a

caisson. The bridge was a handsome edifice, with elliptical arches, and stood well for a few years; but an extraordinary flood occurred, which caused the water to rise five feet higher above than below the bridge, and to flow through it with so great a velocity, as to undermine the piers, and cause the bridge to divide longitudinally, and fall in against the stream; a circumstance so much the more mortifying to the eminent engineer who had constructed it, as it was the only one of his works that, "in a period of thirty years," had been known to fail. It was observed that some of the piers, which had been built in coffer-dams, with the assistance of some piles, withstood the violence of the flood; and it is remarkable, that the whole bridge has been rebuilt by a provincial architect with perfect success, having stood without any accident for many years.

It seems, therefore, scarcely prudent to trust any very heavy bridge to a foundation not secured by piles, unless the ground on which it stands is an absolute rock; and in this case, as well as when piles are to be driven and sawed off, it is generally necessary to have recourse to a coffer-dam. In the instance of the bridge at Harraton, for example, where the rock is nine feet below the bed of the river, Mr. Smeaton directs that the piles forming the coffer-dam be rebated into each other, driven down to the rock, and secured by internal stretchers, before the water contained within them is pumped out. In some cases, a double row of piles, with clay between them, has been employed for forming a coffer-dam; but in others it has been found more convenient to drive and cut off the piles under water, by means of proper machinery, without the assistance of a coffer-dam.

Piles are employed of various lengths, from 7 to 16 feet or more, and from 8 to 10 inches in thickness, and they are commonly shod with iron. Smeaton directs them to be driven till it requires from 20 to 40 strokes of the pile driver to sink them an inch, according to the magnitude of the weight, and the firmness required in the work. He was in the habit of frequently recommending the piles surrounding the piers to be secured by throwing in rubble stone, so as to form an inclined surface, sloping gradually from the bridge upwards and downwards. In the case of Coldstream Bridge, it was also found

necessary to have a partial dam, or artificial shoal, thrown across the river a little below the bridge, in order to lessen the velocity of the water, which was cutting up the gravel from the base of the piles. But all these expedients are attended with considerable inconvenience, and it is better to avoid them in the first instance by leaving the water-way as wide and as deep as possible, and by making the foundations as firm and extensive as the circumstances may require.

The angles of the piers, both above and under water, are commonly rounded off, in order to facilitate the passage of the stream, and to be less liable to accidental injury. Mr. Smeaton recommends a cylindrical surface of 60° as a proper termination; and two such surfaces, meeting each other in an angle, will approach to the outline of the head of a ship, which is calculated to afford the least resistance to the water gliding by it.

We find that, in the year 1769, the earth employed for filling up the space between the walls of the North Bridge in Edinburgh, had forced them out, so as to require the assistance of transverse bars and buttresses for their support. In the more modern bridges, these accidents are prevented by the employment of longitudinal walls for filling up the haunches, with flat stones covering the intervals between them, instead of the earth, or the more solid materials which were formerly used, and which produced a greater pressure both on the arch and on the abutments, as well as a transverse thrust against the side walls. For the inclination of the road passing over this bridge, Mr. Smeaton thought a slope of 1 in 12 not too great; observing that horses cannot trot even when the ascent is much more gradual than this, and that if they walk, they can draw a carriage up such a road as this without difficulty: and, indeed, the bridge at Newcastle appears, for a short distance, to have been much steeper. But it has been more lately argued, on another occasion, that it is a great inconvenience in a crowded city, to have to lock the wheel of a loaded waggon; that this is necessary at all times on Holborn Hill, where the slope is only 1 in 18; while in frosty weather this street is absolutely impassable for such carriages: and the descent of Ludgate Hill, which is only 1 in 36, is considered as much more desirable,

when it is possible to construct a bridge with an acclivity so gentle.

SECTION V.—*Improvements of the Port of London.*

From the study of Mr. Smeaton's diversified labours, we proceed to take a cursory view of the Parliamentary Inquiry respecting the improvement of the Port of London, which has brought forward a variety of important information, and suggested a multiplicity of ingenious designs. The principal part of that which relates to our present subject is contained in the Second and Third Reports from the Select Committee of the House of Commons, on the improvement of the Port of London; ordered to be printed 11th July, 1799, and 28th July, 1800.

We find in these Reports some interesting details respecting the history of London Bridge, which appears to have been begun, not, as Hume tells us, by William Rufus, who was killed in 1100, but in 1176, under Henry II.; and to have been completed in 83 years. The piles are principally of elm, and they have remained for six centuries without material decay; although a part of the bridge fell, and was rebuilt about 100 years after it was begun. Rochester, York, and Newcastle Bridges were also built in the twelfth century, as well as the Bridge of St. Esprit at Avignon. About 50 years ago, the middle pier of London Bridge was removed; the piles were drawn by a very powerful screw, commonly used for lifting the wheels of the water-works; and a single arch was made to occupy the place of two. In consequence of this, the fall was somewhat diminished, and it was necessary partially to obstruct the channel again, in order that the stream should have force enough for the water-works; but it was very difficult to secure the bottom from the effects of the increased velocity under the arch. Several strong beams were firmly fixed across the bed of the river, but only two of them retained their situations for any length of time; and the materials carried away had been deposited below the middle arch, so as to form a shoal, which was only 16 inches below the surface at low water. The Reports contain also much particular information respecting

Blackfriars Bridge, the piles for which were driven under water, and cut off level with the bed of the foundations, by a machine of Mr. Mylne's invention. The expense of Blackfriars Bridge, including the purchase of premises, was about 260,000*l.*; that of the building only was 170,000*l.* Westminster Bridge, built in the beginning of the century, cost about 400,000*l.*

The committee had received an immense variety of plans and proposals for docks, wharfs, and bridges, and many of these have been published in the Reports, together with engraved details on a very ample scale. They finally adopted three resolutions respecting the rebuilding of London Bridge.

"1. That it is the opinion of this Committee, that it is essential to the improvement and accommodation of the port of London, that London Bridge should be rebuilt upon such a construction as to permit a free passage, at all times of the tide, for ships of such a tonnage, at least, as the depth of the river would admit of, at present, between London Bridge and Blackfriars Bridge.

"2. That it is the opinion of this Committee, that an iron bridge, having its centre arch not less than 65 feet high in the clear, above high-water-mark, will answer the intended purpose, and at the least expense.

"3. That it is the opinion of this Committee, that the most convenient situation for the New Bridge will be immediately above St. Saviour's Church, and upon a line from thence to the Royal Exchange."

In a subsequent Report, ordered to be printed 3rd June, 1801, we find a plan for a magnificent iron bridge of 600 feet span, which had been submitted to the Committee by Messrs. Telford and Douglas. Mr. Telford's reputation in his profession as an engineer deservedly attracted the attention of the Committee; but many practical difficulties having been suggested to them, they circulated a number of queries relating to the proposal, among such persons of science, and professional architects, as were the most likely to have afforded them satisfactory information. But the results of these inquiries are not a little humiliating to the admirers of abstract reasoning and of geometrical evidence; and it would be difficult to find a

greater discordance in the most heterodox professions of faith, or in the most capricious variations of taste, than is exhibited in the responses of our most celebrated professors, on almost every point submitted to their consideration. It would be useless to dwell on the numerous errors with which many of the answers abound; but the questions will afford us a very convenient clue for directing our attention to such subjects of deliberation as are really likely to occur in a multiplicity of cases; and it will perhaps be possible to find such answers for all of them, as will tend to remove the greater number of the difficulties which have hitherto embarrassed the subject.

QUESTIONS RESPECTING THE CONSTRUCTION OF A CAST IRON BRIDGE, OF A SINGLE ARCH, 600 FEET IN THE SPAN, AND 65 FEET RISE. (Plate, Fig. 7.)

I. *What parts of the bridge should be considered as wedges, which act on each other by gravity and pressure, and what parts as weight, acting by gravity only, similar to the walls and other loading, usually erected upon the arches of stone bridges? Or does the whole act as one frame of iron, which can only be destroyed by crushing its parts?*

The distribution of the resistance of a bridge may be considered as in some measure optional, since it may be transferred from one part of the structure to another, by wedging together most firmly those parts which we wish to be most materially concerned in it. But there is also a natural principle of adjustment, by which the resistance has a tendency to be thrown where it can best be supported; for the materials being always more or less compressible, a very small change of form, supposed to be equal throughout the structure, will relieve those parts most which are the most strained, and the accommodation will be still more effectual when the parts most strained undergo the greatest change of form. Thus, if the flatter ribs, seen at the upper part of the proposed structure, supported any material part of its weight, they would undergo a considerable longitudinal compression, and being shortened a little, would naturally descend very rapidly upon the more curved, and consequently

stronger parts below, which would soon relieve them from the load improperly allotted to them; the abutment would also give way a little, and be forced out, by the greater pressure at its upper part, while the lower part remained almost entirely unchanged.

It is, however, highly important that the work should, in the first instance, be so arranged as best to fulfil the intended purposes, and especially that such parts should have to support the weight as are able to do it with the least expense of lateral thrust, which is the great evil to be dreaded in a work of these gigantic dimensions, the materials themselves being scarcely ever crushed when the arch is of a proper form; and the failure of an iron bridge, by the want of ultimate resistance of its parts to a compressing force, being a thing altogether out of our contemplation; and it is obvious that the greater the curvature of the resisting parts, the smaller will be the lateral thrust on the abutments.

We may, therefore, sufficiently answer this question, by saying, that the whole frame of the proposed bridge, so far as it lies in or near the longitudinal direction of the arch, may occasionally cooperate in affording a partial resistance if required; but that the principal part of the force ought to be concentrated in the lower ribs, not far remote from the intrados.

But it is by no means allowable to calculate upon a curve of equilibrium exactly coinciding with the intrados; since, if this supposition were realized, we should lose more than three-fourths of the strength of our materials, and all the stability of the joints independent of cohesion, so that the slightest external force might throw the curve beyond the limits of the joint, and cause it to open. Nor can we always consider the curve of equilibrium as parallel to the intrados: taking, for example, the case of a bridge like Blackfriars, the curve of equilibrium, passing near the middle of the arch-stones, is, and ought to be, nine or ten feet above the intrados at the abutment, and only two or three feet at the crown; so that the ordinates of this curve are altogether different from the ordinates which have hitherto been considered by theoretical writers. It may be imagined that this difference is of no great importance in

practice; but its amount is much greater than the difference between the theoretical curves of equilibrium, determined by calculation, and the commonest circular or elliptical arches.

With respect to the alternative of comparing the bridge with masonry or with carpentry, we may say, that the principles on which the equilibrium of bridges is calculated, are altogether elementary, and independent of any figurative expressions of strains and mechanical purchase, which are employed in considering many of the arrangements of carpentry, and which may indeed, when they are accurately analysed, be resolved into forces opposed and combined in the same manner as the thrusts of a bridge. It is, therefore, wholly unnecessary, when we inquire into the strength of such a fabric, to distinguish the thrusts of masonry from the strains of carpentry, the laws which govern them being not only similar but identical; except that a strain is commonly understood as implying an exertion of cohesive force, and we have seen that a cohesive force ought never to be called into action in a bridge, since it implies a great and unnecessary sacrifice of the strength of the materials employed. If, indeed, we wanted to cross a mere ditch, without depending on the firmness of the bank, we might easily find a beam of wood or a bar of iron strong enough to afford a passage over it, unsupported by any abutment, because, in a substance of inconsiderable length, we are sure of having more strength than we require. But to assert that an iron bridge of 600 feet span "is a lever exerting a vertical pressure only on the abutments," is to pronounce a sentence from the lofty tribunal of refined science, which the simplest workman must feel to be erroneous. But, in this instance, the error is not so much in the comparison with the lever, as in the inattention to the mode of fixing it: for a lever or beam of the dimensions of the proposed bridge, lying loosely on its abutments, would probably be at least a hundred times weaker than if it were firmly connected with the abutments as a bridge is, so as to be fixed in a determinate direction. And the true reason of the utility of cast iron for building bridges, consists not, as has often been supposed, in its capability of being united so as to act like a frame of

carpentry, but in the great resistance which it seems to afford to any force tending to crush it.

QUESTION II. *Whether the strength of the arch is affected, and in what manner, by the proposed increase of its width towards the two extremities or abutments, when considered vertically and horizontally? And if so, what form should the bridge gradually acquire?*

The only material advantage, derived from widening the bridge at the ends, consists in the firmness of the abutments; and this advantage is greatly diminished by the increase of horizontal thrust which is occasioned by the increase of breadth; while the curve of equilibrium is caused to deviate greatly from a circular or parabolic arc, in consequence of the great inequality of the load on the different parts; and there seems to be no great difficulty in forming a firm connexion between a narrow bridge and a wider abutment, without this inconvenience. The lateral strength of the fabric, in resisting any horizontal force, would be amply sufficient, without the dilatation at the ends. Perhaps the form was suggested to the inventor by the recollection of the partial failure of an earlier work of the same kind, which has been found to deviate considerably from the vertical plane in which it was originally situated; but in this instance, there seems, if we judge from the engravings which have been published, to have been a total deficiency of oblique braces; and the abutments appear to have been somewhat less firm than could have been desired, since one of them contains an arch and some warehouses, instead of being composed of more solid masonry.

QUESTION III. *In what proportion should the weight be distributed from the centre to the abutments, to make the arch uniformly strong?*

This question is so comprehensive, that a complete answer to it would involve the whole theory of bridges; and it will be necessary to limit our investigations to an inquiry whether the structure, represented in the plan, is actually such as to afford a uniform strength, or whether any alterations can be made in

it, compatible with the general outlines of the proposal, to remedy any imperfections which may be discoverable, in the arrangement of the pressure.

There is an oversight in some of the official answers to this question, from quarters of the very first respectability, which requires our particular attention. The weight of the different parts of the bridge has been supposed to differ so materially from that which is required for producing an equilibrium in a circular arch of equable curvature, that it has been thought impossible to apply the principles of the theory in any manner to an arch so constituted, at the same time that the structure is admitted to be tolerably well calculated to stand, when considered as a frame of carpentry. The truth is, that it is by no means absolutely necessary, nor often perfectly practicable, that the mean curve of equilibrium should agree precisely in its form with the curves limiting the external surfaces of the parts bearing the pressure, especially when they are sufficiently extensive to admit of considerable latitude within the limits of their substance. It may happen in many cases, that the curve of equilibrium is much flatter in one part, and more convex in another, than the circle which approaches nearest to it: and yet the distance of the two curves may be inconsiderable, in comparison with the thickness of the parts capable of co-operating in the resistance. The great problem, therefore, in all such cases, is, to determine the precise situation of the curve of equilibrium in the actual state of the bridge; and when this has been done, the directions of the ribs, in the case of an iron bridge, and of the joints of the arch-stones, in a stone bridge, may be so regulated as to afford the greatest possible security; and if this security is not deemed sufficient, the whole arrangement must be altered.

Considering the effect of the dilatation at the ends in increasing the load, we may estimate the depth of the materials causing the pressure at the abutments as about three times as great as at the crown; the plan not being sufficiently minute to afford us a more precise determination; and it will be quite accurate enough to take $w = a + bx^2$ (Prop. S.) for the load, w becoming $= 3a$ when x is 300 feet, whence $90000\ b = 2a$,

and $b = \frac{1}{45,000}a$; we have then $my = \frac{1}{2}ax^2 + \frac{1}{540,000}ax^4$ for the value of the ordinate. Now the obliquity to the horizon being inconsiderable, this ordinate will not ultimately be much less than the whole height of the arch; and its greatest value may be called 64 feet: consequently when $x = 300$, we have $64\,m = \frac{1}{2}a \times 90,000 + \frac{1}{6}a \times 90,000$, and the radius of curvature at the vertex $r = \frac{m}{a} = 937.5$ feet, while the radius of the intrados is 725 feet, and that of a circle passing through both ends of the curve of equilibrium, as we have supposed them to be situated, 735 feet. Hence, y being $= \frac{1}{1875}x^2\left(1 + \frac{1}{270000}x^2\right)$, we may calculate the ordinates at different points, and compare them with those of the circular curves.

Distance x.	Versed sine of the intrados.	Versed sine of the circular arc.	Ordinate y.
50	1.73	1.71	1.34
100	6.94	6.82	5.38
150	15.66	15.43	13.00
200	28.13	27.70	24.50
250	44.42	43.81	41.01
300	65.00	64.00	64.00

Hence it appears that, at the distance of 200 feet from the middle, the curve of equilibrium will rise more than 3 feet above its proper place; requiring a great proportion of the pressure to be transferred to the upper ribs, with a considerable loss of strength, for want of a communication approaching more nearly to the direction of the curve. If we chose to form the lower part of the structure of two series of frames, each about 4 feet deep, with diagonal braces, we might provide amply for such an irregularity in the distribution of the pressure; but it would be necessary to cast the diagonals as strong as the blocks, in order to avoid the inequality of tension from unequal cooling, which is often a cause of dangerous accidents; it would, however, be much better to have the arch somewhat elliptical in its form, if the load were of necessity such as has been supposed.

QUESTION IV. *What pressure will each part of the bridge receive, supposing it divided into any given number of equal sections, the weight of the middle section being given? And on what parts, and with what force, will the whole act upon the abutments?*

It appears from the preceding calculations. that the weight of the "middle section" alone is not sufficient for determining the pressure in any part of the fabric; although, when the form of the curve of equilibrium has been found, its radius of curvature at the summit must give at once the length of a similar load equivalent to the lateral thrust; and by combining this thrust with the weight, or with the direction of the curve, the oblique thrust at any part of the arch may be readily found. Thus, since at the abutment $w = a + bx^2 = 3a$, and $bx^2 = 2a$, we have $y = \frac{1}{2}\,\frac{a}{m}\,x^2 + \frac{1}{12}\,\frac{b}{m}\,x^4$, and $\frac{dy}{dx}$, the tangent of the inclination, becomes $= \frac{a}{m}x + \frac{1}{3}\,\frac{b}{m}\,x^3 = \frac{ax}{m} + \frac{2}{3}\,\frac{ax}{m} = \frac{5}{3}\,\frac{x}{r} = \frac{5}{3}\cdot\frac{300}{937.5} = \frac{8}{15} = .5333$; consequently the horizontal thrust will be to the weight of the half arch as 15 to 8, and to that of the whole arch as 15 to 16. Now the arch is supposed to contain 6500 tons of cast iron, and together with the road, will amount, according to Professor Robison's estimate, to 10,000 tons; so that the lateral thrust on each abutment is 9470 tons; and since this is equal to the weight of 937.5 feet in length, of the thickness of the crown, the load there must be about 10 tons for each foot of the length. Hence it appears, that although the thrust, thus calculated, is greater than the weight of a portion of equal length with the apparent radius at the crown, it is less than would be inferred from the angular direction of the intrados at the abutment: the inclination of the termination of the arch being 24° 27′, while that of the true curve of equilibrium is 28° 4′; that is, about one-tenth greater.

As a further illustration of the utility of this mode of computation, we may take the example of an arch of Blackfriars Bridge. The radius of curvature, as far as four-fifths of the breadth, is here 56 feet; and we may suppose, without sensible error, the whole load to be that which would be determined by

the continuation of the same curve throughout the breadth. Now, the middle of the arch stones, at the distance of 50 feet from the middle of the bridge, that is, immediately over the termination of the abutment, is about 12 feet above that termination, and at the crown about three feet above the intrados, so that we have only 31 feet for the extreme value of y, while the whole height of the arch is 40; and a being 6·58 feet, we find (Prop. U.) $my = 13{,}510 = 31m$, whence $m = 436$, and $\frac{m}{a} = r = 66\frac{1}{4}$; we also obtain the values of the ordinates of the curve as in the annexed table.

Distance x.	Ordinate y.	Middle of the Arch-stones.
10 feet	.76	.90
20	3.12	3.72
25	5.13	6.12
30	7.71	8.75
40	15.81	16.81
50	31.00	31.00

Hence it appears that the greatest deviation is about 30 feet from the middle, where it amounts to a little more than a foot. But if we suppose this deviation divided by a partial displacement of the curve at its extremities, as it would probably be in reality, even if the resistance were confined to the arch-stones, it would be only about half as great in all three places; and even this deviation will reduce the strength of the stones to two-thirds, leaving them however still many times stronger than can ever be necessary. The participation of the whole fabric, in supporting a share of the oblique thrust, might make the pressure on the arch-stones somewhat less unequal, and the diminution of their strength less considerable; but it would be better that the pressure should be confined almost entirely to the arch-stones, as tending less to increase the horizontal thrust, which is here compressed by $m = 436$, implying the weight of so many square feet of the longitudinal section of the bridge; while, if we determined it from the curvature of the intrados, it would appear to be only $56a = 368$.

In this calculation, the oblique direction of the joints, as

affecting the load, has not been considered; but its effect may be estimated by merely supposing the specific gravity of the materials to be somewhat increased. Thus, since the back of each arch-stone is about one-eighth wider than its lower end, the weight of the materials pressing on it will be about one-sixteenth greater than would press on it, if it were of uniform thickness; and this increase will be very nearly proportional to w, the whole load at each part; so that it will only affect the total magnitude of the thrust, which, instead of 436, must be supposed to amount to about 463. If also great accuracy were required, it would be necessary to appreciate the different specific gravities of the various materials constituting the load; since they are not altogether homogeneous; but so minute a calculation is not necessary in order to show the general distribution of the forces concerned, and the sufficiency of the arrangement for answering all the purposes intended.

Question V. *What additional weight will the bridge sustain, and what will be the effect of a given weight placed upon any of the before-mentioned sections?*

When a weight is placed on any part of a bridge, the curve of equilibrium must change its situation more or less, according to the magnitude of the weight; and the tangent of its inclination must now be increased by a quantity proportional to the additional pressure to be supported, which, if the weight were placed in the middle of the arch, would always be equal to half of it; but when the weight is placed at any other part of the arch, if we find the point where the whole thrust is horizontal, the vertical pressure to be supported at each point of the curve must obviously be equal to the weight of the materials interposed between it and this new summit of the curve. Now, in order to find where the thrust is horizontal, we must divide the arch into two such portions, that their difference, acting at the end of a lever of the length of half the span, that is, of the distance from the abutment, may be equivalent to the given weight, acting on a lever equal to its distance from the other abutment, to which it is nearest; consequently this difference must be to the weight as the distance of the weight from the

end to half the span; and the distance of the new summit of the curve from the middle must be such, that the weight of materials intercepted between it and the middle shall be to the weight as the distance of the weight from the end to the whole span; and the tangent of the inclination must everywhere be increased or diminished by the tangent of the angle at which the lateral thrust would support the weight of this portion of the materials; except immediately under the weight, where the two portions of the curve will meet in a finite angle, at least if we suppose the weight to be collected in a single point.

If, for example, a weight of 100 tons, equal to that of about 10 feet of the crown of the arch, be placed halfway between the abutment and the middle; then the vertex of the curve, where the thrust is horizontal, will be removed $2\frac{1}{2}$ feet towards the weight; but the radius being 937·5 feet, the tangent of the additional inclination will be $\frac{2.5}{937.5} = \frac{1}{375}$, and each ordinate of the curve will be increased $\frac{1}{375}$ of the absciss, reckoning from the place of the weight to the remoter abutment; but between the weight and the nearest abutment, the additional pressure at each point will be $10-2.5 = 7.5$ feet, consequently the tangent will be $\frac{1}{125}$, and the additions to the ordinates at the abutments will be $\frac{450}{375}$ and $\frac{150}{125}$, each equal to $1\frac{1}{5}$ foot, and at the summit $\frac{150}{375} = \frac{2}{5}$, which, being deducted, the true addition to the height of the curve will appear to be $\frac{4}{5}$. But the actual height will remain unaltered, since the curve is still supposed to be terminated by the abutments, and to pass through the middle of the key-stone; and we have only to reduce all the ordinates in the proportion of 64.8 to 64. Thus, at 200 feet from the summit, the ordinate, instead of $24.50 + \frac{200}{375} = 25.03$, will be 24.72, so that the curve will be brought $2\frac{1}{4}$ inches nearer to the intrados, which, in the proposed fabric, would by no means diminish its strength; while on the opposite side, immediately under the weight, the

ordinate $13 - \frac{150}{375} = 12.6$ will be reduced to 12.45, and the curve raised between six and seven inches, which is a change by no means to be neglected in considering the resistances required from each part of the structure. We ought also, if great accuracy were required, to determine the effect of such a weight in increasing the lateral thrust, which would affect in a slight degree the result of the calculation; but it would not amount in the case proposed, to more than one-eightieth of the whole thrust.

It is obvious that the tendency of any additional weight, placed near the middle of a bridge, is to straighten the two branches of the curve of equilibrium, and that, if it were supposed infinite, it would convert them into right lines; provided, therefore, that such right lines could be drawn without coming too near the intrados at the haunches, the bridge would be in no danger of giving way, unless either the materials were crushed, or the abutments were forced out. In fact, any bridge well constructed might support a load at least equal to its own weight, with less loss of strength than would arise from some such errors, as have not very uncommonly been committed, even in works which have on the whole succeeded tolerably well.

QUESTION VI. *Supposing the bridge executed in the best manner, What horizontal force will it require, when applied to any particular part, to overturn it, or press it out of the vertical plane?*

If the bridge be well tied together, it may be considered as a single mass, standing on its abutments; its mean breadth being about 80 feet, and its weight 10,100 tons; and such a mass would require a lateral pressure at the crown of the arch of about 7000 tons to overset it. Any strength of attachment to the abutments would, of course, make it still firmer, and any want of connexion between the parts weaker; and since the actual resistance to such a force must depend entirely on the strength of the oblique connection between the ribs, it is not easy to define its magnitude with accuracy; but, as Professor Robison has justly remarked, the strength would be increased

by causing the braces to extend across the whole breadth of the half arch. The single ribs, if wholly unconnected, might be overset by an inconsiderable force, since they stand in a kind of tottering equilibrium; and something like this appears to have happened to the bridge at Wearmouth. Dr. Hutton, indeed, mentions some "diagonal iron bars" in this bridge, but these were perhaps added after its first erection, to obviate the "twisting" which had become apparent, since they are neither exhibited in the large plates of the bridge, nor mentioned in the specification of the patent.

QUESTION VII. *Supposing the span of the arch to remain the same, and to spring ten feet lower, What additional strength would it give the bridge? Or, making the strength the same, What saving may be made in the materials? Or, if, instead of a circular arch, as in the plates and drawings, the bridge should be made in the form of an elliptical arch, What would be the difference in effect, as to strength, duration, convenience, and expenses?*

The question seems to suppose the weight of the materials to remain unaltered, and the parts of the structure that would be expanded to be made proportionally lighter; which could not be exactly true, though there might be a compensation in some other parts. Granting, however, the weight to be the same under both circumstances, if the ordinate y at the end be increased in the proportion of 64 to about 73, the curvature at the vertex will be increased, and the lateral thrust diminished in the same ratio, the 9470 tons being reduced to 8300. The additional thrust occasioned by any foreign weight would also be lessened, but not the vertical displacement of the curve derived from its pressure; and since the whole fabric might safely be made somewhat lighter, the lightness would again diminish the strain. The very least resistance that can be attributed to a square inch of the section of a block of cast iron, is about 50 tons, or somewhat more than 100,000 pounds. It is said, indeed, that Mr. William Reynolds found, by accurate experiments, that 400 tons were required to crush a cube of a quarter of an inch of the kind of cast iron called gun-metal,

which is equivalent to 6400 tons for a square inch of the section. But this result so far exceeds any thing that could be expected, either from experiment or from analogy, that it would be imprudent to place much reliance on it in practice; the strength attributed to the metal being equivalent to the pressure of a column 2,280,000 feet in height, which would compress it to about four-fifths of its length, since the height of the modulus of elasticity (Prop. G.) is about 10,000,000 feet. The greatest cohesive force that has ever been observed in iron or steel, does not exceed 70 tons for a square inch of the section, and the repulsive force of a homogeneous substance has not been found, in any other instance, to be many times greater or less than the cohesive. There cannot, however, be any doubt that the oblique thrust, which amounts to 10,730 tons, would be sufficiently resisted by a section of 215 square inches, or, if we allowed a load amounting to about one-third only of the whole strength, by a section of 600 square inches; and since each foot of an iron bar, an inch square, weighs three pounds, and the whole length of the arch nearly a ton, the 600 square inches would require nearly as many tons to be employed in the ribs affording the resistance, upon this very low estimate of the strength of cast iron. The doubts here expressed respecting Mr. Reynolds's results have been fully justified by some hasty experiments, which have been obligingly made by the son of a distinguished architect: he found that two parallelepipeds of cast iron, one-eighth of an inch square, and a quarter of an inch long, were crushed by a force of little more than a ton. The experiments were made in a vice, and required considerable reductions for the friction. The mode of calculation may deserve to be explained, on account of its utility on other similar occasions. Supposing the friction to be to the pressure on the screw as 1 to m, and the pressure on the screw to the actual pressure on the substance as n to 1, calling this pressure x, the pressure on the screw will be nx, and the friction $\frac{nx}{m}$; but this resistance will take from the gross ultimate pressure f a force, which is to the friction itself, as the velocity of the parts sliding on each other is to the velocity of the part pro-

ducing the ultimate pressure, a proportion which we may call p to 1; and the force remaining will be the actual pressure: that is. $f - \frac{pnx}{m} = x$, and $x = \frac{m}{m+pn} f$. In these experiments the gross force f, as supposed to be exerted on the iron, was 4 tons: the friction $\frac{1}{m}$, was probably about $\frac{1}{4}$, the screw not having been lately oiled; the distance of the screw from the centre of motion was to the length of the whole vice as 3 to 4, whence n was $\frac{4}{3}$, and p was 8.44, the middle of the screw describing 4 22 inches, while the cheek of the vice moved through $\frac{1}{2}$ an inch: consequently $\frac{m}{m+pn}$ was $\frac{4}{4+11.25} = \frac{1}{3.81}$, and the corrected pressure becomes $\frac{4}{3.81}$. In several experiments made with still greater care, and with an improved apparatus of levers, the mean force required to crush a cube of a quarter of an inch was not quite $4\frac{1}{2}$ tons, instead of 400.

Calcareous freestone supports about a ton on a square inch, which is equal to the weight of a column not quite 2000 feet in height; consequently an arch of such freestone, of 2000 feet radius, would be crushed by its own weight only, without any further load; and for an arch like that of a bridge, which has other materials to support, 200 feet is the utmost radius that it has been thought prudent to attempt; although a part of the bridge of Neuilly stands, cracked as it is, with a curvature of 250 feet radius; and there is no doubt that a firm structure, well arranged in the beginning, might safely be made much flatter than this, if there were any necessity for it.

An elliptical arch would certainly approach nearer to the form of the curve of equilibrium, which would remain little altered by the change of that of the arch; and the pressure might be more equably and advantageously transmitted through the blocks of such an arch, than in the proposed form of the structure. The duration would probably be proportional to the increased firmness of the fabric, and the greater flatness at the crown might allow a wider space for the passage of the masts of large ships on each side of the middle. There might be some additional trouble and expense in the formation of

portions of an elliptical curve; but even this might be in a great measure avoided by employing portions of three circles of different radii, which would scarcely be distinguishable from the ellipsis itself.

Those who have imagined that a circular arch must in general be "stronger than an elliptical arch of the same height and span," have not adverted to the distinction between the apparent curvature of the arch and the situation of the true curve of equilibrium, which depends on the distribution of the weight of the different parts of the bridge, and by no means on the form of the arch-stones only; this form being totally insufficient to determine the true radius of curvature, which is immediately connected with the lateral thrust, and with the strength of the fabric.

Question VIII. *Is it necessary or advisable to have a model made of the proposed bridge, or any part of it, in cast iron? If so, what are the objects to which the experiments should be directed; to the equilibration only, or to the cohesion of the several parts, or to both united, as they will occur in the intended bridge?*

Experiments on the equilibration of the arch would be easy and conclusive; on the cohesion or connection of the parts, extremely uncertain; the form and proportion of the joints could scarcely be imitated with sufficient accuracy; and since the strength of some of the parts concerned, would vary as the thickness simply, and that of others as the square or cube of the thickness, it would be more difficult to argue from the strength of the model upon that of the bridge, than to calculate the whole from still more elementary experiments. Some such experiments ought, however, to be made, on the force required to crush a block of the substance employed; and the form calculated to afford the proper equilibrium, might be very precisely and elegantly determined by means of the method first suggested by Dr. Hooke, that of substituting for the blocks, resting on each other and on the abutments, as many similar pieces forming a chain, and suspended at the extremities. It would, however, be important to make one alteration in the common mode of performing this experiment, without which

it would be of little or no value; the parts corresponding to the blocks of the arch should be formed of their proper thickness and length, and connected with each other and with the abutments by a short joint or hinge in the middle of each, allowing room for a slight degree of angular motion only; and every other part of the structure should be represented in its proper form and proportion and connexion, that form being previously determined as nearly as possible by calculation; and then, if the curve underwent no material alteration by the suspension, we should be sure that the calculation was sufficiently correct; or, if otherwise, the arrangement of the materials might be altered, until the required curve should be obtained; and the investigation might be facilitated by allowing the joints or hinges connecting the block to slide a little along their surfaces, within such limits as would be allowable, without too great a reduction of the powers of resistance of the blocks.

Question IX. *Of what size ought the model to be made, and what relative proportions will experiments, made on the model, bear to the bridge when executed?*

The size is of little importance, and it would be unsafe to calculate the strength of the bridge from any general comparison with that of the model. There is an *Essay* of Euler in the 'New Commentaries of the Royal Academy of Petersburgh,' (Vol. xx. p. 271), relating expressly to the mode of judging of the strength of a bridge from a model; but it contains only an elementary calculation, applicable to ropes and simple levers, and by no means comprehending all the circumstances that require to be considered in the structure of an arch.

Question X. *By what means may ships be best directed in the middle stream, or prevented from driving to the side, and striking the arch; and what would be the consequence of such a stroke?*

For the direction of ships, Professor Robison's suggestion seems the simplest and best, that they might be guided by means of a small anchor, dragged along the bottom of the river. The stroke of a ship might fracture the outer ribs, if

they were too weak, but could scarcely affect the whole fabric in any material degree, supposing it to be firmly secured by oblique bars, crossing from one side of the abutment to the other side of the middle; and if still greater firmness were wanted, the braces might cross still more obliquely, and be repeated from space to space.

A ship moving with a velocity of three miles in an hour, or about four feet in a second, would be stopped by a force equal to her weight, when she had advanced three inches with a retarded motion; and the bridge could not very easily withstand, at any one point, a force much greater than such a shock of a large ship, if it were direct, without being dangerously strained. But we must consider that a large ship could never strike the bridge with its full force, and that the mast would be much more easily broken than the bridge. The inertia of the parts of the bridge, and of the heavy materials laid on it, would enable it to resist the stroke of a small mass with great mechanical advantage. Thus the inertia of an anvil, laid on a man's chest, enables him to support a blow on the anvil, which would be fatal without such an interposition, the momentum communicated to the greater weight being always less than twice the momentum of the smaller; and this small increase of momentum being attended by a much greater decrease of energy or impetus, which is expressed by the product of the mass into the square of the velocity, and which is sometimes called the ascending or penetrating force, since the height of ascent or depth of penetration is proportional to it, when the resistance is given; and the same mode of reasoning is applicable to any weight falling on the bridge, or to any other cause of vibration which is not likely to call forth in such a fabric any violent exertion of the strength of the parts, or of their connections. We must also remember, in appreciating the effect of a stroke of any kind on an arched structure, that something of strength is always lost by too great stiffness; the property of resisting velocity, which has sometimes been called resilience, being generally diminished by any increase of stiffness, if the strength, with respect to pressure, remains the same.

Question XI. *The weight and lateral pressure of the bridge being given, can abutments be made in the proposed situation for London Bridge, to resist that pressure?*

Since this question relates entirely to the local circumstances of the banks of the Thames, the persons to whom it has been referred have generally appealed to the stability of St Saviour's Church, in a neighbouring situation, as a proof of the affirmative; and it does not appear that there have been any instances of a failure of piles well driven, in a moderately favourable soil. Professor Robison, indeed, asserts that the firmest piling will yield in time to a pressure continued without interruption; but a consideration of the general nature of friction and lateral adhesion, as well as the experience of ages in a multitude of structures actually erected, will not allow us to adopt the assertion as universally true. When, indeed, the earth is extremely soft, it would be advisable to unite it into one mass for a large extent, perhaps as far as 100 yards in every direction, for such a bridge as that under discussion, by beams radiating from the abutments, resting on short piles, with cross pieces interspersed; since we might combine, in this manner, the effect of a weight of 100,000 tons, which could scarcely ever produce a lateral adhesion of less than 20,000, even if the materials were semifluid; for they would afford this resistance if they were capable of standing in the form of a bank, rising only one foot in five of horizontal extent, which any thing short of an absolute quicksand or a bog would certainly do in perfect security. The proper direction of the joints of the masonry may be determined for the abutment exactly as for the bridge, the tangent of the inclination being always increased in proportion to the weights of the successive wedges added to the load; and the ultimate inclination of the curve is that in which the piles ought to be driven; being the direction of the result, composed of the lateral thrust, combined with the joint weight of the half bridge and the abutment.

Question XII. *The weight and lateral pressure of the bridge being given, can a centre or scaffolding be erected over the river*

sufficient to carry the arch without obstructing the vessels which at present navigate that part?

There seems to be no great difficulty in the construction of such a centre. When the bridge at Wearmouth was erected, the centre was supported by piles and standards, which suffered ships to pass between them without interruption, and a similar arrangement might be made in the present case with equal facility.

QUESTION XIII. *Whether would it be most advisable to make the bridge of cast and wrought iron combined, or of cast iron only? And if of the latter, Whether of the hard white metal, or of the soft grey metal, or of gun metal?*

A bridge well built ought to require no cohesive strength of ties, as Mr. Southern has justly observed in his answer to the eighth question; and for repulsive resistance, in the capacity of a shore, cast iron is probably much stronger than wrought. It has also the advantage of being less liable to rust, and of expanding somewhat less by heat than wrought iron; but wherever any transverse strain is unavoidable, wrought iron possesses some advantages, and it is generally most convenient for bolts and other fastenings. The kind of iron called gun metal is decidedly preferred by the most experienced judges, as combining in the greatest degree the properties of hardness and toughness; the white being considered as too brittle, and the grey as too soft. Dr. Hutton, however, and Mr. Jessop, prefer the grey; and if we allow the strength of the gun metal to be at all comparable to that which Mr. Reynolds attributes to it, we must also acknowledge that a much weaker substance would be amply sufficient for every practical purpose, and might deserve to be preferred, if it were found to possess a greater degree of tenacity.

QUESTION XIV. *Of what dimensions ought the several members of the iron work to be, to give the bridge sufficient strength?*

See the answers to Questions VII. and XI.

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QUESTION XV. *Can frames of cast iron be made sufficiently correct to compose an arch of the form and dimensions shown in the drawings, so as to take an equal bearing as one frame, the several parts being connected by diagonal braces, and joined by an iron cement, or other substance?*

Professor Robison considers it as indispensable that the frames of cast iron should be ground to fit each other; and a very accurate adjustment of the surface would certainly be necessary for the perfect co-operation of every part of so hard a substance. Probably, indeed, any very small interstices that might be left, would in some measure be filled up by degrees, in consequence of the oxydation of the metal, but scarcely soon enough to assist in bearing the general thrust upon the first completion of the bridge. The plan of mortising the frames together is by no means to be advised, as rendering it very difficult to adapt the surfaces to each other throughout any considerable part of their extent. They might be connected either as in the bridge at Wearmouth, by bars of wrought iron let into the sides, which might be of extremely moderate dimensions; or, as in some still more modern fabrics, by being wedged into the grooves of cross plates adapted to receive them, which very effectually secure the co-operation of the whole force of the blocks, and which have the advantage of employing cast iron only.

QUESTION XVI. *Instead of casting the ribs in frames of considerable length and breadth, would it be more advisable to cast each member of the ribs in separate pieces of considerable lengths, connecting them together by diagonal braces, both horizontally and vertically?*

No joint can possibly be so strong as a single sound piece of the same metal; and it is highly desirable that the curve of pressure should pass through very substantial frames or blocks, abutting fully on each other, without any reliance on lateral joints; but for the upper parts of the work, single ribs, much lighter than those which form the true arch, would be sufficiently firm.

QUESTION XVII. *Can an iron cement be made, which shall become hard and durable, or can liquid iron be poured into the joints?*

Mr. Reynolds has observed, that a cement composed of iron borings and saline substances, will become extremely hard; and it is probable that this property depends on the solidity which is produced by the gradual oxydation of the iron. It would certainly be injurious to the strength of the fabric to interpose this cement between perfectly smooth and solid surfaces; but it might be of advantage to fill up with it any small interstices unavoidably left between the parts. To pour melted iron into the joints would be utterly impracticable.

QUESTION XVIII. *Would lead be better to use in the whole or any part of the joints?*

Lead is by far too soft to be of the least use, and a saline cement would be decidedly preferable.

QUESTION XIX. *Can any improvements be made in the plan, so as to render it more substantial and durable, and less expensive; And if so, what are these improvements?*

The most necessary alterations appear to be the omission of the upper and flatter ribs; the greater strength and solidity of the lower, made either in the form of blocks or of frames with diagonals; a curvature more nearly approaching to that of the curve of equilibrium, and a greater obliquity of the cross-braces.

It would be necessary to wedge the whole structure very firmly together before the removal of the centres, a precaution which is still more necessary for stone bridges, in which a certain portion of soft mortar must inevitably be employed, in order to enable the stones to bear fully on each other, and which has been very properly adopted in the best modern works. In this manner we may avoid the inconvenience pointed out by Professor Robison, who has remarked, that the compressibility of the materials, hard as they appear, would occasion a reduction of three inches in the length of the bridge, from the effect of the lateral thrust, and a consequent fall at the crown of 15; a result which will not be found materially erroneous, if the cal-

culation be repeated from more correct elements, derived from later experiments and comparisons. For obviating the disadvantageous effects of such a depression, which he seems to have supposed unavoidable, as well as those of a change of temperature, which must in reality occur, though to a less considerable extent, Professor Robison suggested the expedient of a joint in the middle of the bridge, with an intermediate portion, calculated to receive the rounded ends of the opposite ribs, somewhat like an interarticular cartilage; but it is impossible to devise any kind of joint, without limiting the pressure, during the change of form, to a very small portion of the surfaces, which could not bear fully on each other throughout their extent, if any such liberty of motion were allowed, unless all friction between them were prevented; and a similar joint would be required at the abutment, where it would be still more objectionable, as extending to a wider surface.

The arrangement of the joints between the portions of the ribs, in one or more transverse lines, would be a matter of great indifference. Some have recommended to break the joints, as is usual in masonry, in order to tie the parts more firmly together; others to make all the joints continuous, as a safer method, on account of the brittleness of the materials; but if the fabric were well put together, there would be neither any want of firm connection, nor any danger of breaking from irregular strains, in whatever way the joints might be disposed.

QUESTION XX. *Upon considering the whole circumstances of the case, agreeable to the Resolutions of the Committee, as stated at the conclusion of their Third Report, is it your opinion that an arch of* 600 *feet in the span, as expressed in the drawings produced by Messrs. Telford and Douglas, or the same plan, with any improvement you may be so good as to point out, is practicable and advisable, and capable of being made a durable edifice?*

The answers that have been returned to this question are almost universally in the affirmative, though deduced from very discordant and inconsistent views of the subject. The only reasonable doubt relates to the abutments; and with the precautions which have been already mentioned in the answer to the

11th question, there would be no insuperable difficulty in making the abutments sufficiently firm.

QUESTION XXI. *Does the estimate, communicated herewith, according to your judgment, greatly exceed or fall short of the probable expense of executing the plan proposed: specifying the general grounds of your opinion?*

The estimate amounts to 262,289*l.*; and it has generally been considered as below the probable expense. The abutments are set down at 20,000*l.*; but they would very possibly require five times as much, to be properly executed; while some other parts of the work, by a more judicious distribution of the forces concerned, might safely be made so much lighter, as considerably to lessen the expense of the whole fabric, without any diminution either of its beauty or of its stability.

No. LIII.

A PORTION OF THE ARTICLE

'CARPENTRY,'

From the Supplement to the Encyclopædia Britannica, vol. ii. p. 621.

It has been judged most expedient to reprint the article Carpentry from the *Supplement* to the *third* edition of the 'Encyclopædia,' in order that it may form, with the articles Roof and Strength of Materials, a uniform system of the most useful departments of practical mechanics, deduced, in the same familiar and elementary manner, from the simple principles of the composition of forces: premising some Introductory Observations, which may be considered as a retrospective summary of the doctrine of Passive Strength, accompanied by some of the most useful propositions respecting the resistance of elastic substances, derived from the principles which have been already laid down in our article Bridge: and subjoining a few notes, on such passages as may appear to require further illustration or correction. Some of the demonstrations will be partly borrowed from a work which has been published since the death of Professor Robison, the able author of these three articles: but others will be more completely original: and of the remarks, the most important will probably be those which relate to the form and direction of the abutments of rafters; a subject which seems to have been very incorrectly treated by former writers on Carpentry.

I. Abstract of the Doctrine of Passive Strength.

The effects of forces of different kinds, on the materials employed in the mechanical arts, require to be minutely examined in the arrangement of every work dependent on them; and of

these effects, as exhibited in a solid body at rest, we may distinguish seven different varieties: the extension of a substance acting simply as a tie; the compression of a block supporting a load above it; the detrusion of an axis resting on a support close to its wheel, and resisting by its lateral adhesion only; the flexure of a body bent by a force applied unequally to its different parts; the torsion or twisting, arising from a partial detrusion of the external parts in opposite directions, while the axis retains its place; the alteration or permanent change of a body which settles, so as to remain in a new form, when the force is withdrawn; and lastly, the fracture, which consists in a complete separation of parts before united, and which has been the only effect particularly examined by the generality of authors on the strength of materials.

The analogy of the laws of extension and compression has been demonstrated in a former article* of this volume, and their connexion with flexure has been investigated: but it is not easy to compare them directly with the resistance opposed to a partial detrusion, the effects of which are only so far understood as they are exhibited in the phenomena of twisting: and these appear to justify us in considering the resistance of lateral adhesion as a primitive force, deduced from the rigidity or solidity of the substance, and proportional to the deviation from the natural situation of the particles. The resistance exhibited by steel wire, when twisted, bears a greater proportion to that of brass than the resistance to extension or compression; but the forces agree in being independent of the hardness produced by tempering.

Flexure may be occasioned either by a transverse or by a longitudinal force: when the force is transverse, the extent of the flexure is nearly proportional to its magnitude; but when it is longitudinal, there is a certain magnitude which it must exceed, in order to produce or rather to continue the flexure, if the force be applied exactly at the axis. But it is equally true that the slightest possible force applied at a distance from the axis, however minute, or with an obliquity however small, or to a beam already a little curved, will produce a certain

* Supra, p. 195.

degree of flexure; and this observation will serve to explain some of the difficulties and irregularities which have occurred, in making experiments on beams exposed to longitudinal pressure.

Stiffness, or the power of resisting flexure, is measured by the force required to produce a given minute change of form. For beams similarly fixed, it is directly proportional to the breadth and the cube of the depth, and inversely to the cube of the length. Thus a beam or bar two yards long will be equally stiff with a beam one yard, provided that it be either twice as deep, or eight times as broad. If the ends of a beam can be firmly fixed, by continuing them to a sufficient distance, and keeping them down by a proper pressure, the stiffness will be four times as great as if the ends were simply supported. A hollow substance, of given weight and length, has its stiffness nearly proportional to the square of the diameter: and hence arises the great utility of tubes, when stiffness is required, this property being still more increased by the expansion of the substance than the ultimate strength. It is obvious that there are a multiplicity of cases in Carpentry where stiffness is of more importance than any other property, since the utility as well as beauty of the fabric might often be destroyed by too great a flexibility of the materials.

If we wish to find how much a beam of fir will sink when it is loaded in the middle, we may multiply the cube of the length in inches by the given weight in pounds, and divide by the cube of the depth, and by ten million times the breadth: but on account of the unequal texture of the wood, we must expect to find the bending somewhat greater than this in practice, besides that a large weight will often produce an alteration, or permanent settling, which will be added to it: a beam of oak will also sink a little more than a beam of fir, with the same weight.

With respect to torsion, the stiffness of a cylindrical body varies directly as the fourth power of the diameter, and inversely in the simple proportion of the length: it does not appear to be changed by the action of any force tending to lengthen or to compress the cylinder: and it may very possibly bear some simple relation to the force of cohesion, which has not yet been fully

ascertained: but it appears that, in an experiment of Mr. Cavendish, the resistance of a cylinder of copper to a twisting force, acting at its surface, was about $\frac{1}{16}$ of the resistance that the same cylinder would have opposed to direct extension or compression.

Alteration is often an intermediate step between a temporary change and a complete fracture. There are many substances, which, after bending to a certain extent, are no longer capable of resuming their original form: and in such cases it generally happens that the alteration may be increased without limit, until complete fracture takes place, by the continued operation of the same force which has begun it, or by a force a little greater. Those substances which are the most capable of this change, are called ductile, and the most remarkable are gold and a spider's web. When a substance has undergone an alteration by means of its ductility, its stiffness, in resisting small changes on either side, remains little or not at all altered. Thus if the stiffness of a spider's web, in resisting torsion, were sufficient at the commencement of an experiment, to cause it to recover itself, after being twisted in an angle of ten degrees, it would return ten degrees, and not more, after having been twisted round a thousand times. The ductility of all substances, capable of being annealed, is greatly modified by the effects of heat: hard steel, for example, is incomparably less subject to alteration than soft, although in some cases more liable to fracture: so that the degree of hardness requires to be proportioned to the uses for which each instrument is intended: although it was proved by Coulomb, and has since been confirmed by other observers, that the primitive stiffness of steel, in resisting small flexures, is neither increased nor diminished by any variation in its temper.

The strength of a body is measured by the force required completely to overcome the corpuscular powers concerned in the aggregation of its particles, and it is jointly proportional to the primitive stiffness, and to the toughness of the substance; that is, to the degree in which it is capable of a change of form without permanent alteration. It becomes however of importance in some cases, to consider the measure of another kind of

strength, which has sometimes been called resilience, or the power of resisting a body in motion, and which is proportional to the strength and the toughness conjointly, that is, to the stiffness and the square of the toughness. Thus if we double the length of a given beam, we reduce its absolute strength to one half, and its stiffness to one eighth; but since the toughness, or the space, through which it will continue to resist, is quadrupled, the resilience will be doubled, and it would require a double weight to fall from the same height, or the same weight to fall from a double height, in order to overcome its whole resistance. If we wish to determine the resilience of a body from an experiment on its strength, we must measure the distance through which it recedes or is bent, previously to its fracture; and it may be shown that a weight, which is capable of breaking it by pressure, would also break it by impulse if it moved with the velocity acquired by falling from a height equal to half the deflection. Thus if a beam or bar were broken by a weight of 100 pounds, after being bent 6 inches without alteration, it would also be broken by a weight of 100 pounds falling from a height of 3 inches, or moving in a horizontal direction with a velocity of 4 feet in a second, or by a weight of 1 pound falling from a height of 300 inches. This substitution of velocity for quantity of matter has however one limit, beyond which the velocity must prevail over the resistance, without regard to the quantity of matter, and this limit is derived from the time required for the successive propagation of the pressure through the different parts of the substance, in order that they may participate in the resistance. Thus if a weight fell on the end of a bar or column with the velocity of 100 feet in a second, and the substance could only be compressed $\frac{1}{200}$ of its length, without being crushed, it is obvious that the pressure must be propagated through the substance with a velocity of 20,000 feet in a second, in order that it might resist the stroke; and, in general, a substance will be crushed or penetrated by any velocity exceeding that which is acquired by a body falling from a height, which is to half that of the modulus of elasticity of the substance, as the square of the greatest possible change of length is to the whole length. From the consideration of

the effect of rigidity in lessening the resilience of bodies, we may understand how a diamond, which is capable of resisting an enormous pressure, may be crushed with a blow of a small hammer, moving with a moderate velocity. It is remarkable that, for the same substance in different forms, the resilience is in most cases simply proportional to the bulk or weight, while almost every other kind of resistance is capable of infinite variation by change of form only.

The elaborate investigations of Mr. Lagrange, respecting the strength and the strongest forms of columns, appear to have been conducted upon principles not altogether unexceptionable; but it is much easier to confute the results than to follow the steps of the computations. One great error is the supposition that columns are to be considered as elastic beams, bent by a longitudinal force; while, in reality, a stone column is never slender enough to be bent by a force which it can bear without being crushed; and even for such columns as are capable of being bent by a longitudinal force, Mr. Lagrange's determinations are in several instances inadmissible; he asserts, for example, that a cylinder is the strongest of all possible forms, and that a cone is stronger than any conoid of the same bulk; but it appears to be demonstrable in a very simple manner, and upon incontestable principles, that a conoidal form may be determined, which shall be stronger than either a cone or a cylinder of the same bulk.

When a column is crushed, its resistance to compression seems to depend in great measure on the force of lateral adhesion, assisted by a kind of internal friction, dependent on the magnitude of the pressure, and it commonly gives way by the separation of a wedge in an oblique direction. If the adhesion were simply proportional to the section, it may be shown that a square column would be most easily crushed when the angle of the wedge is equal to half of a right angle; but, if the adhesion is increased by pressure, this angle will be diminished by half the angle of repose appropriate to the substance. In a wedge separated by a direct force from a prism of cast iron, the angle was found equal to $32\frac{1}{2}^\circ$, consequently the angle of repose was $2 \times 12\frac{1}{2}^\circ = 25^\circ$, and the internal friction to the pressure as

1 to .466, the tangent of this angle: there was, however, a little bubble in the course of the fracture, which may have changed its direction in a slight degree. The magnitude of the lateral adhesion is measured by twice the height of the wedge, whatever its angle may be: in this instance the height was to the depth as 1.57 to 1, consequently the surface, affording an adhesion equal to the force, was somewhat more than three times as great as the transverse section, and the lateral adhesion of a square inch of cast iron would be equal to about 46,000 pounds; the direct cohesive force of the same iron was found by experiment equal to about 20,000 pounds for a square inch. It is obvious that experiments on the strength of a substance in resisting compression ought to be tried on pieces rather longer than cubes, since a cube would not allow of the free separation of a single wedge so acute as was observed in this experiment; although, indeed, the force required to separate a shorter wedge on each side would be little or no greater than for a single wedge. The same consideration of the oblique direction of the plane of easiest fracture would induce us to make the outline of a column a little convex externally, as the common practice has been: for a circle cut out of a plank possesses the advantage of resisting equally in every section, and consequently of exhibiting the strongest form, when there is no lateral adhesion; and in the case of an additional resistance proportional to the pressure, the strongest form is afforded by an oval consisting of two circular segments, each containing twice the angle formed by the plane of fracture with the horizon. If we wish to obtain a direct measure of the lateral adhesion, we must take care to apply the forces concerned at a distance from each other not greater than one sixth of the depth of the substance, otherwise the fracture will probably be rather the consequence of flexure than of detrusion. Professor Robison found this force in some instances twice as great as the direct cohesion, or nearly in the same proportion, as it appears to have been in the experiment on the strength of cast iron; Mr. Coulomb thinks it most commonly equal only to the cohesion: and in fibrous substances, especially where the fibres are not perfectly straight, the repulsive strength is generally much less than would be inferred

from this equality, and sometimes even less than the cohesive strength.

It is well known that the transverse strength of a beam is directly as the breadth, and as the square of the depth, and inversely as the length: and the variation of the results of some experiments from this law can only have depended on accidental circumstances. If we wish to find the number of hundred weights that will break a beam of oak, supported at both ends, supposing them to be placed exactly on the middle, we may multiply the square of the depth, in inches, by 100 times the breadth, and divide by the length; and we may venture in practice to load a beam with at least an eighth as much as this, or in case of necessity, even a fourth. And if the load be distributed equally throughout the length of the beam, it will support twice as much: but for a beam of fir, the strength is somewhat less than for oak. A cylinder will bear the same curvature as the circumscribing prism, and it may be shown that its strength, as well as its stiffness, is to that of the prism as one fourth of its bulk is to one third of the bulk of the prism. The strength of a beam supported at its extremities may be doubled by firmly fixing the ends, where it is practicable; and we have already seen that the stiffness is quadrupled: but the resilience remains unaltered, since the resistance is doubled, and the space through which it acts is reduced to a half. It is therefore obviously of importance to consider the nature of the resistance that is required from the fabric which we are constructing. A floor, considered alone, requires to be strong; but in connexion with a ceiling, its stiffness requires more particular attention. in order that the ceiling may remain free from cracks. A coach spring requires resilience, for resisting the relative motions of the carriage, and we obtain this kind of strength as effectually by combining a number of separate plates, as if we united them into a single mass, while we avoid the stiffness, which would render the changes of motion inconveniently abrupt.

In all calculations respecting stiffness, it is necessary to be acquainted with the modulus of elasticity, which may be found, for a variety of substances, in the annexed table.

HEIGHT OF THE MODULUS OF ELASTICITY IN THOUSANDS OF FEET.

Iron and steel . . .	10,000	Fir wood	10,000
Copper	5,700	Elm	8,000
Brass	5,000	Beech	8,000
Silver	3,240	Oak	5,060
Tin	2,250	Box	5,050
Crown glass . . .	9,800	Ice	850

II. PROPOSITIONS RELATING TO FLEXURE.

A. *The stiffness of a cylinder is to that of its circumscribing rectangular prism as three times the bulk of the cylinder is to four times that of the prism.*

We may consider the different strata of the substance as acting on levers equal in length to the distance of each from the axis; for although there is no fixed fulcrum at the axis, yet the whole force is the same as if such a fulcrum existed, since the opposite actions of the opposite parts would relieve the fulcrum from all pressure. Then the tension of each stratum being also as the same distance x, and the breadth of the stratum being called $2y$, the fluxion of the force on either side of the axis will be $2x^2y\mathrm{d}x$, while that of the force of the prism, the radius being r, is $2rx^2\mathrm{d}x$. Now z being the area of half the portion included between the stratum and the axis, of which the fluxion is $y\mathrm{d}x$, the fluxion of $z - \frac{y^3x}{rr}$ will be $y\mathrm{d}x - \frac{y^3\mathrm{d}x}{rr} - \frac{3y^2x\mathrm{d}y}{rr}$, or since $\frac{xx}{rr} = 1 - \frac{yy}{rr}$, $\frac{xxy\mathrm{d}x}{rr} - \frac{3y\mathrm{d}yx}{r}\left(-\frac{x\mathrm{d}x}{y}\right)$, or $\frac{xxy\mathrm{d}x}{rr} + \frac{3xxy\mathrm{d}x}{rr} = \frac{4xxy\mathrm{d}x}{rr}$; consequently the fluent of ${}^2x^2y\mathrm{d}x$ is $\frac{1}{2}r^2z - \frac{1}{2}y^3x$, which, when $y = 0$ becomes $\frac{1}{2}r^2z$, or one fourth of the product of the square of the radius by the area of the section, while the fluent of $2rx^2\mathrm{d}x$, that is, $\frac{2}{3}rx^3$, the force of the prism, becomes $\frac{2}{3}r^4$ or $\frac{1}{3}r^2 \times 2r^2$, one third of the product of the same square into the area of the section of the prism.

Hence the radius of curvature of a cylindrical column, instead of $\frac{Maa}{12fy}$ (Art. BRIDGE, Prop. G.*) will be $\frac{Maa}{16fy}$, the weight of the modulus M decreasing in the same proportion as the bulk, when the prism is reduced to a cylinder. The force is sup-

* Supra, p. 200.

posed in this proposition to be either transverse or applied at a considerable distance from the axis: but the error will not be material in any other case.

B. *When a longitudinal force f is applied to the extremities of a straight prismatic beam, at the distance b from the axis, the deflection of the middle of the beam will be* $b\left(\text{SECANT } [\sqrt{(\frac{3f}{M})} \cdot \frac{e}{a}] - 1\right)$; M *being the weight of the modulus, e the length of the beam, and a its depth.*

The curvature being proportional to the distance from the line of direction of the force, or to the ordinate, when that line is considered as the abscissa, the elastic curve must, in this case, initially coincide with a portion of the harmonic curve, well known for its utility in the resolution of a variety of problems of this kind. Now if the half length of the complete curve be called k, corresponding to a quadrant of the generating circle, and the greatest ordinate y, c being the quadrant of a circle of which the radius is unity, the radius of curvature r corresponding to y will be $\frac{kk}{ccy}$, that is, a third proportional to y and $\frac{k}{c}$ the radius of the generating circle; consequently $\frac{Maa}{12fy} = \frac{kk}{ccy}$, $kk = \frac{Maacc}{12f}$, and $k = \frac{1}{2}\sqrt{\frac{M}{3f}} \cdot ac$; but, by the nature of the curve, $y : b = 1 : \cos. \frac{ec}{2k} = \text{SEC.} \frac{ec}{2k} : 1$ and $y = b \text{ SEC.} \frac{ec}{2k} = b \text{ SEC.} \sqrt{\frac{3f}{M}} \cdot \frac{e}{a}$, which is the ordinate at the middle; and the deflection from the natural situation is $y - b$.

It follows that, since the secant of the quadrant is infinite when $\sqrt{\frac{3f}{M}} \cdot \frac{e}{a}$ becomes equal to c, the deflection will be infinite, and the resistance of the column will be overcome, however small the distance b may be taken, provided that it be of finite magnitude: and since in this case $\frac{3fee}{Maa} = cc$, $f = \frac{Maacc}{3ee} = .8225 \text{ M} \frac{aa}{ee}$, which is the utmost force that the column will bear: and for a cylinder we find, by the same reasoning, $f = \frac{Maacc}{4ee} = .6169 \text{ M} \frac{aa}{ee}$. If b be supposed to

vanish, we shall have in theory an equilibrium without flexure, but since it will be tottering, it cannot exist in nature.

By applying this determination to the strength of wood and iron, compared with the modulus of elasticity, it appears, that a round column or a square pillar of either of these substances cannot be bent by any longitudinal force applied to the axis, which it can withstand without being crushed, unless its length be greater than 12 or 13 times its thickness respectively: nor a column or pillar of stone, unless it be 40 or 45 times as long as it is thick. Hence we may infer, as a practical rule, that every piece of timber or iron, intended to withstand any considerable compressing force, should be at least as many inches in thickness as it is feet in length, in order to avoid the loss of force which necessarily arises from curvature.

C. *When a beam, fixed at one end, is pressed by a force in a direction deviating from the original position of the axis in a small angle, of which the tangent is t, the deflection becomes* $d = at \sqrt{\frac{M}{12f}}\ \text{TANG.}\left(\sqrt{\frac{12f}{M}} \cdot \frac{e}{a}\right)$.

The inclination of the curve to the absciss being inconsiderable, it will not differ sensibly from a portion of a harmonic curve; and supposing the quadrantal length of this curve k, we have again, as in the last proposition, $k = \frac{1}{2}\sqrt{\frac{M}{3f}} \cdot ac$, or for a cylinder, $k = \frac{1}{4}\sqrt{\frac{M}{f}} \cdot ac$. Now, the tangent of the inclination of the harmonic curve varies as the sine of the angular distance from the middle, consequently as sin. $\frac{k-e}{k} \cdot c$, or cos. $\frac{ec}{k}$ is to the radius, so is the tangent t expressing the difference of inclination of the end of the beam and the direction of the force, which is also that of the middle of the supposed curve, to the tangent of the extreme inclination of the curve to its absciss, which will therefore be t SEC. $\frac{ec}{k}$: consequently the greatest ordinate will be $\frac{kt}{c}$ SEC. $\frac{ec}{k}$, and since the ordinates are as the sines of the angular distances from the

origin of the curve, the ordinate at the fixed end of the beam, corresponding to the angle $\frac{ec}{k}$, that is, the deflection, will be $\frac{kt}{c}$ SEC. $\frac{ec}{k}$. sin. $\frac{ec}{k} = \frac{kt}{c}$ TANG. $\frac{ec}{k} = \frac{1}{2}\sqrt{\frac{M}{3f}}$ *at* TANG. $\frac{2e}{a}\sqrt{\frac{3f}{M}}$, or, for a cylinder, $\frac{1}{4}\sqrt{\frac{M}{f}}$. *at* TANG. $\frac{4e}{a}\sqrt{\frac{f}{M}}$.

By means of this proposition we may determine the effect of a small lateral force in weakening a beam or pillar, which is at the same time compressed longitudinally by a much greater force; considering the parts on each side of the point, to which the lateral force is applied, as portions of two beams, bent in the manner here described, by a single force slightly inclined to the axis.

D. *A bar fixed at one end, and bent by a transverse force applied to the other end, assumes initially the form of a cubic parabola, and the deflection at the end is* $d = \frac{4e^3f}{Maa}$.

The ordinate of a cubic parabola varying as x^3, its second fluxion varies as $6x\,(\mathrm{d}x)^2$, or since the first fluxion of the absciss is constant, simply as the absciss x, measured from the vertex of the parabola, which must therefore be situated at the end to which the force is applied, and the absciss must coincide with the tangent of the bar. But if we begin from the other end, we must substitute $e-x$ for x, and the second fluxion of the ordinate will be as $6\,(e-x)\,(\mathrm{d}x)^2$, the first as $6ex\mathrm{d}x - 3x^2\mathrm{d}x$, and the fluent as $3ex^2 - x^3$, which, when $x = e$, becomes $2e^3$, while it would have been $3e^3$ if the curvature had been uniform, and the second fluxion had been everywhere $6e(\mathrm{d}x)^2$. Now the radius of curvature at the fixed end being $r = \frac{Maa}{12ef}$, and the versed sine of a small portion of a circle being equal to $\frac{ee}{2r}$, this versed sine will be expressed by $\frac{6e^3f}{Maa}$; and two thirds of this, or $\frac{4e^3f}{Maa}$, will be the actual deflection.

E. *The depression of a bar, fixed horizontally at one end, and supporting only its own weight, is* $d = \frac{3e^4}{2maa}$; *m being the height of the modulus of elasticity.*

The curvature here varies as the square of the distance from the end, because the strain is proportional to the weight of the portion of the bar beyond any given point, and to the distance of its centre of gravity conjointly, that is, to $(e-x)\ \frac{1}{2}(e-x)$, so that if the second fluxion at the fixed end be as $e^2(\mathrm{d}x)^2$, it will elsewhere be as $(e-x)^2\ (\mathrm{d}x)^2$; and the corresponding first fluxions being $e^2x\mathrm{d}x$ and $e^2x\mathrm{d}x - ex^2\mathrm{d}x + \frac{1}{3}x^3\mathrm{d}x$, the fluents will be $\frac{1}{2}e^2x^2$, and $\frac{1}{2}e^2x^2 - \frac{1}{3}ex^3 + \frac{1}{12}x^4$, or when $x = e$, $\frac{1}{2}e^4$, and $(\frac{1}{2} - \frac{1}{3} + \frac{1}{12})\ e^4 = \frac{1}{4}e^4$; consequently the depression must be half the versed sine in the circle of greatest curvature. Now the radius of curvature $\frac{Maa}{12fy}$ becomes here $\frac{Maa}{6ef}$, the force being applied at the distance $\frac{1}{2}e$: and since the weight of the bar is to that of the modulus of elasticity in the proportion of the respective lengths, we have $\frac{f}{M} = \frac{e}{m}$, and $r = \frac{maa}{6ee}$, and the versed sine for the ordinate e will be $\frac{3e^4}{maa}$, half of which is the actual depression.

F. *The depression of the middle of a horizontal bar, fixed at both ends, and supporting its own weight only, is* $d = \frac{5e^4}{32maa}$.

The transverse force at each point of such a bar, resisted by the lateral adhesion, is as the distance x from the middle (Art. BRIDGE, Prop. L. p. 205); but this force is proportional to the first fluxion of the strain or curvature, consequently the curvature itself must vary as the corrected fluent of $\pm\ x\mathrm{d}x$, taking here the negative sign, because the curvature diminishes as x increases; and the corrected fluent will be $\frac{1}{4}e^2 - x^2$, since it must vanish when $x = \frac{1}{2}e$; the first fluxion of the ordinate will then be $\frac{1}{4}e^2x\mathrm{d}x - \frac{1}{3}x^3\mathrm{d}x$, and the fluent $\frac{1}{8}e^2x^2 - \frac{1}{12}x^4$, or for the whole length $\frac{1}{2}e$, $\frac{5}{192}e^4$, instead of $\frac{1}{32}$, or $\frac{6}{192}$, which would have been its value if the curvature had been equal throughout. Now the strain at the middle is the difference of the opposite strains produced by the forces acting on either side; and these are the half weight, acting at the mean distance $\frac{1}{4}e$, and the resistance of the support, which is equal to the same half weight, but acts at the distance $\frac{1}{2}e$, the difference being equivalent to the half weight acting at the distance $\frac{1}{4}e$, so that the curvature at the

middle is the same as if the bar were fixed there, and loose at the ends, that is, as in the last proposition, substituting $\frac{1}{2}e$ for e, $r = \frac{2maa}{3ee}$; and the versed sine at the distance $\frac{1}{2}e$ being $\frac{e^2}{8r}$, or $\frac{3e^4}{16maa}$, $\frac{5}{6}$ of this will be $\frac{5e^4}{32maa}$. This demonstration may serve as an illustration of two modes of considering the effect of a strain, which have not been generally known, and which are capable of a very extensive application.

It follows that where a bar is equally loaded throughout its length, the curvature at the middle is half as great as if the whole weight were collected there, the strain derived from the resistance of the support remaining in that case uncompensated. The depression produced by the divided weight will be $\frac{5}{8}$ as great as by the single weight, since $\frac{5}{6} \times \frac{1}{2}$ is to $\frac{2}{3}$ as 5 to 8. Mr. Dupin found the proportion by many experiments, between $\frac{2}{3}$ and $\frac{4}{7}$; and $\frac{5}{8}$ is a very good mean for representing these results.

No. LIV.

A THEORY OF TIDES,

INCLUDING THE CONSIDERATION OF RESISTANCE.*

From Nicholson's Journal for 1813, vol. xxxv. pp. 145 and 217.

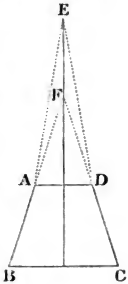
[The author of these investigations is sensible that they are not altogether so explicitly and demonstratively detailed as could be desired; they were not written with any immediate view to publication; but, as they contain some new results, and may possibly lead to new methods of calculation, he thinks it better that they should be printed in an imperfect form, than that they should be wholly lost, which is the only alternative compatible with his present engagements. He does not apologize to an author from whom he has borrowed some ideas, because all those, who are sufficiently interested in the subject

* This article contains the first complete mathematical sketch of Dr. Young's theory of the Tides, though the general principles upon which it rested and the results to which they lead were very precisely stated in his 47th Lecture, as well as in the Journals of the Royal Institution and elsewhere. Some of the more important propositions which it includes were more completely developed in his Elementary Illustrations of the Celestial Mechanics of Laplace; but it was finally presented in its most complete form in the Supplement to the Encyclopædia Britannica, in an article which immediately succeeds the one given in the text.

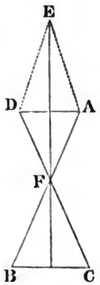
Dr. Young was accustomed to regard his exposition of the theory of the tides as nearly the most successful of his physico-mathematical labours, only second in importance to his researches on the theory of light. The author of the well known article on Waves and Tides in the Encyclopædia Metropolitana, the first living authority on this subject, in answer to some inquiries respecting them which were addressed to him by the editor, thus speaks of them: "You ask my opinion of Dr. Young's researches on tides; as far as they go they are capital: when I was writing my article I totally forgot Dr. Young, although I well knew that in writing on *any* physical subject it is but ordinary prudence to look at him first. When I came to look at him, I was surprised to find that he has clearly enough shown the difference of positive and negative waves, and also the difference of free oscillations and forced oscillations: and that he has hinted at the cause of the rapid rise of river tides as distinguished from their slower fall. All these were great points with me, quite original to myself. There is one of mine, however, which he has not got, namely, the effect of friction in producing an apparent retardation of the day of spring tides, &c."—*Note by the Editor.*

to study this essay, are probably already acquainted with that author's works.]

Theorem A.* If the point of suspension (A) of a pendulum (AB) be made to vibrate in a regular manner, that is, according to the law of cycloidal vibrations, the pendulum itself may also vibrate regularly in the same time, provided that the extent of its vibrations (BC) be to that of the vibrations of the point of suspension (AD) as the length of the thread (AE) supposed to carry this point "as a pendulum," is to the difference of the lengths of the two threads.



In representing the vibrations, we may disregard the curvature of the paths, considering them as of evanescent extent, the forces being however still supposed to depend on the inclination of the threads, "which must be exaggerated in the figures employed." Let F be the intersection of AB with the vertical line EF; then, upon the conditions of the theorem, BF will be equal to AE, that is, if $BC : AD = AE : AE \backsim AB$, since by similar triangles $BC : AD = BF : BF \backsim AB$, it follows that $AE = BF$. Consequently the inclination of the thread AB will always be the same as if F were its fixed point of suspension, and the body B will begin and continue its vibrations like a simple pendulum attached to that point, the true point of suspension accompanying it with a proportional velocity, so as to be always in the right line passing through it and through F. It is obvious, that when the thread supposed to suspend the moveable point of suspension is the longer of the two, the vibrations will be in the same direction; when the shorter, in contrary directions.



* The theorems A and B, with their scholia, are reprinted in the form given to them in the Illustrations of the Celestial Mechanics of Laplace, p. 157. The parts of them which are new, or which differ from the original article, are marked by inverted commas.—*Note by the Editor.*

Scholium 1. The truth of this proposition may easily be illustrated, by holding any pendulous body in the hand, and causing it to vibrate more or less rapidly, by moving the hand regularly backwards or forwards, "in a longer or in a shorter time than that of the spontaneous vibrations."

Scholium 2. The same mode of reasoning is applicable to oscillations of any other kinds, which are governed by forces proportional to the distances of the bodies concerned, from a point of which the situation, either in a quiescent space, or with respect to another moveable point, varies according to the law of the cycloidal pendulum, or may be expressed by the sines of arcs varying with the time: such forces always producing periodical variations, of which the extent is to that of the excursions of the supposed point of suspension in the ratio of n to $n - 1$, n being to 1 as the square of the time of the forced, to that of the time of the spontaneous vibration, and when $n - 1$ is negative, the displacement being in a direction opposite to that of the supposed point of suspension. Consequently, when a body is performing oscillations by the operation of any force, and is subjected to the action of any other periodical forces, we have only to enquire at what distance a moveable point must be situated before or behind it, in order to represent the actual magnitude of the periodical force by the relative situation according to the law of the primary force concerned, and to find an expression for this distance in terms of the sines of arcs increasing equably, in order to obtain the situation and velocity of the body at any time, provided that we suppose it to have attained a permanent state of vibration.

"*Scholium* 3. We may easily express this reasoning in a form more strictly algebraical: thus the time, with respect to the forced vibration of the centre of suspension, being called t, the place of the vertical line passing through that point will be indicated by sin. t, supposing t to begin from the middle of a vibration: now the force acting on the moving body will always be as its distance from this moveable vertical line, considered with relation to the length of the true pendulum m; that is, it will be expressed by $f = \frac{s - \sin. t}{m}$, the unit of m being the

length of the imaginary pendulum carrying the point of suspension, since when $s = 0$, and $\sin. t = 1$, the force must be $= 1$ or $= g$. Now we may satisfy this equation by the particular solution $s - \sin. t = a \sin. t$, which represents a vibration either corresponding in its direction with the former, or in an opposite direction, accordingly as a is positive or negative; and s, the space actually described, will be the sum or difference of the spaces belonging to the separate vibrations so combined: then since $v = -\int f\mathrm{d}t$, and $s = \int v\mathrm{d}t$, we have $v = -\int \frac{a \sin. t}{m} \mathrm{d}t = \frac{a}{m} \cos. t + c$, and $s = \frac{a}{m} \sin. t + ct = a \sin. t + \sin. t$, and $c = 0, \frac{a}{m} = a + 1, a = 1 : (\frac{1}{m} - 1) = \frac{m}{1 - m}$, or if $n = \frac{1}{m}, \frac{1}{n - 1}$, as before."

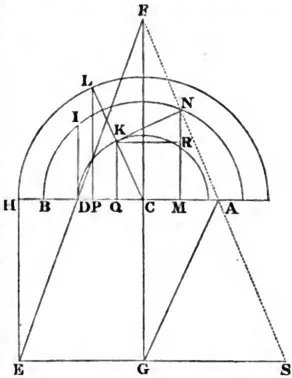
Scholium 4. If the oscillating body be initially in any other condition, its subsequent motion may be determined, by considering it as performing a secondary vibration with respect to a point vibrating in the manner here supposed, which will consequently represent its mean place; but if there be no resistance, the body will have no tendency to assume the form of a regular simple vibration, rather than any other. "Supposing, for example, that the point had been initially at rest in the middle vertical line, when the centre of suspension passed that line; it will then agree in situation with the point representing its mean place, but not in velocity; and it will return to its mean place after every interval equal to a complete single spontaneous vibration of the true pendulum; and when this coincidence happens in the middle vertical line as at first, the whole cycle of motions will begin again, after a period depending on the comparative lengths of the supposed pendulums; and at some intermediate time the coincidence will in most cases occur near the extremity of the vibration representing the mean place, and the excursion will be much greater than that of this vibration, while at another part of the cycle it may be almost obliterated. Such a succession of cycles may be often observed in the actual vibrations of elastic bodies of irregular forms, the excursions being alternately greater and smaller without any interference of external causes.

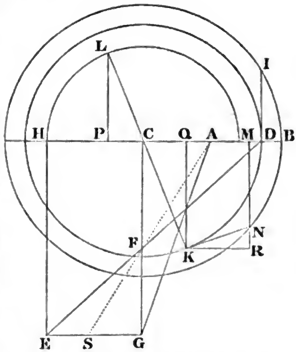
"*Scholium* 5. A more general analytical solution of the problem may be obtained by making $s = b \sin. t + c \sin. (et + h)$ whence $v = -\int f dt = -\int n \left\{ (b-1) \sin. t + c \sin. (et + h) \right\} dt = n \left\{ (b-1) \cos. t + \frac{c}{e} \cos. (et + h) \right\} + i$, since $d \cos. (et + h) = -\sin. (et + h)\, edt$; and $s = \int v dt = n \left\{ (b-1) \sin. t + \frac{c}{ee} \sin. (et + h) \right\} + it + k = b \sin. t + c \sin. (et + h)$; whence $n (b-1) = b$, $\frac{nc}{ee} = c$, $i = 0$ and $k = 0$; consequently $n = \frac{b}{b-1}$, and $b = \frac{1}{n-1}$, $\frac{n}{ee} = 1$, and $e = \sqrt{n}$, h and c remaining altogether undetermined. We may, therefore, accommodate this expression to any relative values of the supposed vibrations, or of the forces belonging to them, and to any conditions of motion or rest in the initial state of the moving body. Thus, if we suppose it initially at rest, so that $s = 0$ and $v = 0$ when $t = 0$, the length n being given, we have $s = b \sin. t + c \sin. (et + h) = 0$, and consequently $h = 0$, and $v = n (b-1) \cos. t + \frac{c}{e} \cos. et = b + \frac{c}{e} = 0$, and $\frac{c}{e} = -b = \frac{-1}{n-1}$ whence $c = \frac{-e}{n-1} = \frac{\sqrt{n}}{1-n}$, and we have $s = \frac{\sin. t}{n-1} + \frac{\sqrt{n}}{n-1} \sin. \sqrt{n}t$."

THEOREM B. If the resistance be simply proportional to the velocity, a pendulum with a vibrating point of suspension may perform regular vibrations, isochronous with those of the point of suspension, provided that, at the middle of a vibration, the point of suspension (A) be so situated as to cause a propelling force equal to the actual resistance, the extent of the vibrations being reduced in the ratio of the whole excursion of the point of suspension (BC) to its distance from the middle at the beginning of the motion of the pendulous body (DC): and it will ultimately acquire this mode of vibration, whatever may have been its initial condition.

Let FG be the supposed length of the thread carrying the point of suspension, and draw FE passing through D " instead of B;" then if HC = EG be the extent of the vibration, it will be maintained according to the law of the cycloidal pen-

dulum. Draw the concentric circles BI, DK, HL: then the initial force may be represented by HD, which determines the greatest inclination of the thread; and at any subsequent part of the vibration, if the centre be advanced from D to M, the time elapsed will be expressed by the arc IN, DI and MN being perpendicular to AB; and taking HL similar to IN, the perpendicular LP will show the place of the pendulous body, and PM the force, which may be divided or resolved into two parts, PQ and QM. But PQ is to LK, or HD, as PC to LC, or HC; consequently this part of the force will always be employed in generating the regular velocity; and QM is equal to KR, which is the sine of the angle KNR or BCL to the radius KN = DI = AC, each of these lines being equal to the sine of BI; the line QM therefore varies as the velocity, and will always be equivalent to the friction, provided that it be once equivalent to it, as it is supposed to be at A; the ratio of the forces concerned, in any two succeeding instants, being always such as to maintain a regular vibration.





If the pendulum be initially in any other situation than that which is here supposed, its subsequent motion may be determined by comparison with that of a point so vibrating, "and its progress may be estimated, with tolerable accuracy, while this deviation exists, by supposing it to perform small vibrations

with respect to its mean place, in which the immediate effect of resistance may be neglected:" but since these vibrations are not supported by any new sustaining force, they will evidently be rendered by degrees smaller and smaller, so that the pendulum will ultimately approach infinitely near to the regular state of vibration here described, which may therefore be considered as affording a stable equilibrium of motion.

Scholium 1. Supposing the relation of the resistance to the velocity to be altered, the relation of the sine AC to the cosine CD must be similarly altered, the force equivalent to the resistance varying as the sine, and the extent of the vibrations, and consequently the velocity, as the cosine of the displacement BI: but the relation of the sine to the cosine is that of the tangent to the radius: so that the tangent of the displacement will be as the mean resistance. And the sine of the displacement AC is to the radius BC as the greatest resistance is to the greatest force which would operate on the pendulous body if it remained at rest at G: "the displacement at the extremity of the vibration having the same angular measure, but becoming, with respect to the place of the body, the verse sine only, instead of the sine.

" *Scholium* 2. It is obvious, from the figures, that the body G will always be behind the place S, which it would have occupied without the resistance, when the vibration is direct, but before it when inverted.

" *Scholium* 3. When the resistance is very small, a simple pendulum with a similar resistance may be conceived to vibrate nearly in a similar manner: and if we neglect the diminution of the velocity in the consideration of the resistance, and call $r = mv = m \cos. t$, we have $v = -\int f\mathrm{d}t = -\int(\sin. t + m \cos. t)\,\mathrm{d}t = \cos. t - m \sin. t$, and $s = \int v\mathrm{d}t = \sin. t + m \cos. t - a = \sqrt{(1 + m^2)} \sin. (t + b) - a$, b being the angle of which the tangent is m, and $a = \sqrt{(1 + m^2)} \sin. b = \sqrt{(1 + m^2)} \frac{m}{\sqrt{(1 + mm)}} = m$; consequently $s = \sqrt{(1+m^2)} \sin. (t+b) - m$, which implies a vibration observing the period of t, but beginning at a point at the distance b further back in the circle, so that the time of ascent will be diminished and that of descent increased very nearly in an equal degree."

Scholium 4. This proposition may also be deduced from the former, by representing the resistance as a force tending to a moveable centre of attraction, analogous to the point of suspension of a pendulum, so as to create a new vibration liable to an equal resistance; or still more simply in the present instance, by attributing the whole actual resistance to the principal vibration, and considering the subordinate vibration as exempt from it. The resistance at G may evidently be represented by the force acting on a pendulum of the length AG at the distance AC from the vertical line, and the corresponding excursion of the pendulous body must be represented, according to the former proposition, by GS, which is to AC as the length of the thread corresponding to the periodical time is to the difference of the lengths: so that when the place of the body, as determined by the former proposition, without resistance, would have been S, it is actually found in G: the centre of attraction representing the resistance being always behind the body, the body will also be behind the place which it would have occupied without the resistance when the vibration is direct, but before it when inverted: and it will be found, that the forces concerned preserve their due proportion in every other part of the vibration. At the beginning of the true vibration, the body must have its greatest velocity in the subordinate vibration representing the effect of resistance, and this velocity must be equal and contrary to the supposed velocity in the primitive vibration, independent of resistance; consequently AC, representing the greatest velocity in the subordinate vibration, must be equal to DI, the sine of the displacement, which shows the velocity in the primitive vibration. And this agreement with the former demonstration is sufficient to show the accuracy of this mode of representing the operation of the forces concerned.

Theorem C. If the resistance be proportional to the square of the velocity, a pendulum with a vibrating point of suspension may perform vibrations isochronous with those of the point of suspension, and very nearly regular, the relative situations being nearly the same as in the case of a similar pendulum liable to

a resistance simply proportional to the velocity, and equal in its aggregate amount to the actual resistance.

The mode of investigation which has been exemplified in the last scholium, may be applied to this and to all other similar cases; the only difficulty being of a mathematical nature, since the method depends on the expression of the forces concerned in the terms of sines or cosines of arcs, and their multiples; and it appears to be frequently impossible to do this otherwise than by approximation. In the present instance we cannot obtain a perfectly correct expression for the square of the sine: the square of the sine, in the common language of mathematics, being always positive, and this case requiring an alternation of positive and negative values, the common forms employed by Euler, Arbogast, and others, completely fail; and the difficulty seems to be rather in the nature of the problem, than in the mode of investigation, for the formula which answers the conditions most completely for one part of the circle, seems to be incorrect at others. Thus we may put $\sin.{}^2 x = a \sin. x + b \sin. 3x + c \sin. 5x \ldots$ omitting the even multiples, since they would afford different values for the corresponding parts of the first two quadrants, and take the successive orders of fluxions, whence we have

$$\sin.{}^2 x = a \sin. x + b \sin 3x + c \sin. 5x + \ldots$$

$$2 \sin. x \cos. x = a \cos. x + 3b \cos. 3x + 5c \cos. 5x + \ldots$$

$$2 - 4 \sin.{}^2 x = - a \sin. x - 9b \sin. 3x - 25c \sin. 5x - \ldots$$

$$-8 \sin. x \cos. x = - a \cos. x - 27b \cos. 3x - 125 \cos. 5x - \ldots$$

$$16 \sin.{}^2 x - 8 = a \sin. x + 81b \sin. 3x + 625c \sin. 5x + \ldots$$

If in these five equations we make x alternately $= 90°$, and $= 0$, we may find five coefficients, $a = \cdot 7861$, $b = - \cdot 2598$, $c = - \cdot 03709$, $d = \cdot 01612$, and $e = \cdot 00732$, which represent the ordinates of a curve agreeing with the curve proposed at the vertex and at the origin in situation, in curvature, and in the first and second fluxions of the curvature: and yet the curves differ surprisingly from each other in the intermediate parts; the ordinate at 45° becoming less than ·4 instead of ·5.

On the whole, the best mode of determining the coefficients, viz. (·4) (·5) appears to be, to divide the quadrant into as many

parts as we wish to have coefficients, and to substitute the corresponding values of the arc in the general equation; we thus obtain $a = \cdot 8484$, $b = -\cdot 1696$, $c = -\cdot 0244$, $d = -\cdot 0081$, $e = -.0029$, and $f = -\cdot 0013$. Then if we make, as before, the square of the time in the entire forced vibration of the point of suspension to the square of the time of the spontaneous vibration of the pendulum as n to 1, the distance of the pendulous body will be expressed by $\frac{n}{n-1}$ when that of the point of suspension is unity; and accordingly as $n-1$ is positive or negative, the body will be on the same side of the vertical line with the point of suspension, or on the opposite side: and the same will be true with respect to the displacement corresponding to the first term of the series expressing the resistance, substituting the supposed centre of attraction for the point of suspension, and the mean place for the vertical line: but in the following terms, the value of n is successively reduced to $\frac{1}{9}$, $\frac{1}{25}$, and so forth: consequently, the whole displacement immediately produced by the effect of the resistance r sin ^2x will be $rn \left(\frac{\cdot 8484}{n-1} \sin. x - \frac{\cdot 1696}{n-9} \sin 3x - \frac{\cdot 0244}{n-25} \sin. 5x - \frac{\cdot 0081}{n-49} \sin. 7x - \frac{\cdot 0029}{n-81} \sin. 9x - \frac{\cdot 0013}{n-121} \sin. 11x\right)$. This displacement will, however, cause an alteration of the resistance, which may be considered as a differential of the former, and since $(y^2)^{\cdot} = 2y\dot{y}$, the new resistance may be expressed by the product of the new and twice the original velocity, or by $-2r^2n \sin. x \left(\frac{\cdot 8484}{n-1} \cos. x - 3 \times \frac{\cdot 1696}{n-9} \cos. 3x - 5 \times \frac{\cdot 0244}{n-25} \cos. 5x - \ldots\right)$ and the consequent displacement may be determined in the same manner as for the original resistance. The first term gives $2r^2n^2 \frac{\cdot 4242}{(n-1)(n-4)} \sin. 2x$: for the remainder we must find an equivalent series in terms of the cosines of multiple arcs, since the direction of the force of resistance does not change where the sine becomes negative: and each term will require a separate investigation while n remains indeterminate; but for the present purpose two of the terms will be sufficient. The method employed by Euler for determining the coefficients

in such cases is of no use here, because it affords a progression of sines only: we must, therefore, put a cos. $x + b$ cos $3x +$ c cos. $5x+d$ cos. $7x+\ldots$ successively equal to sin. x cos. $3x$ and sin. x cos. $5x$; then bisecting and trisecting the quadrant, we find the coefficients ·1667, — ·3333, ·6869, and — ·5202, and —·1057, — ·2886, ·6421, and —·2478, respectively, and the whole of this second displacement becomes $2r^2n^2\Big(\frac{\cdot 4242}{(n-1).(n-4)}$ sin. $2x - \frac{\cdot 5088}{n-9}\big(\frac{\cdot 1667}{n-1}$ cos. $x - \frac{\cdot 3333}{n-9}$ cos. $3x + \frac{\cdot 6869}{n-25}$ cos. $5x$ $- \frac{\cdot 5202}{n-49}$ cos. $7x\big) - \frac{\cdot 1220}{n-23}\big(- \frac{\cdot 1057}{n-1}$ cos. $x - \frac{\cdot 2886}{n-9}$ cos. $3x +$ $\frac{\cdot 6421}{n-25}$ cos. $5x - \frac{\cdot 2478}{n-49}$ cos. $7x\big)\Big)$.

For example, if we take r and n, each equal to $\frac{1}{2}$, the first formula gives —·4434 for the displacement at the middle of the time, and 0 for the beginning; the second 0 at the middle, and — ·0022 at the beginning: but the true beginning of the actual vibration is modified by the velocity belonging to the first order of the effects of resistance, which is found in this case ·390, consequently, the true time of rest will be when the velocity is —·390 in the primitive vibration, or when the arc corresponding to the excursion is 67°, and its sine ·920, which, lessened by ·0022, shews the true extent of the excursion ·9178; and reckoning from this point as the beginning, the displacement in the middle will be reduced to about ·05. Now an equal mean resistance, varying simply in proportion to the velocity, would cause a displacement in the middle of ·3957 instead of 4434; and reckoning from the true beginning of the vibration, the displacement in the middle would vanish, instead of being reduced to about ·05, and the extent would be ·9196 instead of 9178. And if r were smaller than $\frac{1}{2}$, there would obviously be still less difference in the two cases. From the small proportion which the second displacement bears in this case to the first, it may be inferred, that any further calculation of the effects of the third order would be wholly superfluous.

Scholium 1. Dr. T. has suggested an ingenious method, which affords a formula for the coefficients of the first series; but unfortunately it loses its convergence too soon to be of any

use. Taking the equation $2 \sin. x \cos. x = a \cos. x + 3b \cos. 3x + 5c \cos. 5x + \ldots = 2\ (1 - \cos.^2 x)^{-\frac{1}{2}} \cos. x$, we may expand this expression, by means of the binomial theorem, into $2 \cos. x - \cos.^3 x - \frac{1}{4} \cos.^5 x - \frac{1}{8} \cos.^7 x - \ldots$ and substituting the cosines of multiple arcs for the powers of $\cos. x$, and then comparing the homologous terms, we obtain $a = 2\ (1 - \alpha - \beta - \gamma - \delta - \ldots)$ where $\alpha = \frac{3}{16}$, $\beta = \frac{1}{4}.\ \frac{1}{2}.\ \frac{3}{8}\ \alpha$, $\gamma = \frac{1}{4}.\ \frac{3}{3}.\ \frac{7}{4}\ \beta$, $\delta = \frac{1}{4}.\ \frac{5}{4}.\ \frac{9}{5}\ \gamma$, $\epsilon = \frac{1}{4}\ \frac{7}{5}.\ \frac{11}{6}\ \delta \ldots$: $b = -\frac{8}{3}\ (\alpha + \beta + \gamma + \ldots)$ where $\alpha = \frac{1}{16}$, $\beta = \frac{1}{4}.\ \frac{1}{1}.\ \frac{3}{4}\ \alpha$, $\gamma = \frac{1}{4}.\ \frac{3}{2}.\ \frac{7}{5}\ \beta$, $\delta = \frac{1}{4}.\ \frac{5}{3}.\ \frac{9}{6}\ \gamma$: and $c = -\frac{8}{5}\ (\alpha + \beta + \gamma + \ldots)$ where $\alpha = \frac{1}{256}$, $\beta = \frac{1}{4}.\ \frac{3}{1}.\ \frac{7}{6}\ \alpha$, $\gamma = \frac{1}{4}.\ \frac{5}{2}.\ \frac{9}{7}\ \beta, \ldots$: but in all these series it is obvious that the ratio of the terms, as they diminish, approaches to equality; so that it is even difficult to determine whether or no this sum is finite. But a still greater objection to this method is, that the third fluxion $- 8 \sin. x \cos. x$, treated in the same way, affords a very different result.

Scholium 2. In the case of a simple pendulum, subjected for a single vibration to a resistance proportional to the square of the velocity, the space described may be correctly calculated by means of a logarithmic equation, and the time might also be expressed, if it were required, in a series. Let the space described be x, and the resistance y, then the force may be represented by $1 - x - y$, and the square of the velocity will be $\int (\dot{x} - x\dot{x} - y\dot{x})$, whence $y = a \int (\dot{x} - x\dot{x} - \dot{y}x)$, and $\dot{y} = a\dot{x} - ax\dot{x} - ay\dot{x}$, and $\dot{y} + ay\dot{x} = (a - ax)\dot{x}$. Then if we make $y = uz$, z being a function of x, and $\dot{y} = u\dot{z} + z\dot{u}$, we have $u\dot{z} + z\dot{u} + auz\dot{x} = (a - ax)\dot{x}$; and in order to determine z, we may put $u\dot{z} + auz\dot{x} = o$, $\frac{\dot{z}}{z} + a\dot{x} = o$, HL$z = - ax$, or $z = e^{-ax}$, and, substituting for $z\dot{u}$, $e^{-ax}\ \dot{u} = (a - ax)\ \dot{x}$, or, if $ax = w$, $e^{-w}\ \dot{u} = \dot{w} - \frac{w}{a}\ \dot{w}$, $u = e^{w}\ \dot{w} - \frac{1}{a}\ e^{w}\ w\ \dot{w}$, and $y = e^{-w} \int e^{w}\ \dot{w} - \frac{1}{a}\ e^{-w} \int e^{w}\ w\dot{w}$. Hence (Eul. calc. integr. pr. 52) $y = Ce^{-w} + 1 - \frac{1}{a}\ De^{-w} - \frac{1}{a}\ w + \frac{1}{a} = Ce^{-ax} + 1 - \frac{1}{a}\ De^{-ax} +$

$x + \frac{1}{a}$, which must vanish when $x = o$, and $e^{-ax} = 1$, whence $C - \frac{1}{a}D = -\frac{a+1}{a}$, and $y = \frac{a+1}{a}(1 - e^{-ax}) - x$.

Scholium 3. The same result may be still more simply obtained, by repeated approximations, from the first expression $y = a\int(\dot{x} - x\dot{x} - y\dot{x})$: neglecting first the term $y\dot{x}$, we have $y = ax - \frac{1}{2}ax^2$; then substituting this value in $y\dot{x}$, we have $a\int y\dot{x} = \frac{1}{2}a^2x^2 - \frac{1}{2\times 3}a^2x^3$, and $y = ax - \frac{1}{2}ax^2 - \frac{1}{2}a^2x^2 + \frac{1}{2\times 3}a^2x^3$, to which we add, by another similar operation, the terms $+\frac{1}{2\times 3}a^3x^3 - \frac{1}{2.3.4}a^3x^4$, and the whole becomes $ax - \frac{1}{2}a^2x^2 + \frac{1}{2\times 3}a^3x^3 - \frac{1}{2.3.4}a^4x^4 \ldots - \frac{1}{2}ax^2 + \frac{1}{2\times 3}a^2x^3 - \ldots$. But it is well known that $e^{-ax} = 1 - ax + \frac{1}{2}a^2x^2 - \frac{1}{2\times 3}a^3x^3\ldots$; therefore, $y = 1 - e^{-ax} - \frac{1}{a}e^{-ax} + \frac{1}{a} - x$; precisely as before. The length of the whole vibration may obviously be found by making $y = 0$: and if it be desired to determine the time, we must develope, by means of the polynomial theorem, the expression $\frac{\dot{x}}{a}(ax - \frac{1}{2}(a + a^2)x^2 + \frac{1}{2\times 3}(a^2 + a^3)x^3 - \ldots)^{-\frac{1}{2}}$, and take the fluent.

Scholium 4. This mode of exhaustion may be illustrated by another well-known case of a pendulum with a constant resistance, which is known not to alter the time of vibration. Calling the initial force 1, and the resistance a, the velocity destroyed will be ax, if x be the arc proportional to the time: and the diminution of the space will be $\frac{1}{2}ax^2$: but this displacement will cause a new force, which is to the initial force 1 as $\frac{1}{2}ax^2$ to the radius 1, and is therefore represented by $\frac{1}{2}ax^2$; hence the velocity will be increased by the quantity $\int\frac{1}{2}ax^2\dot{x} = \frac{1}{2\times 3}ax^3$, and the space by $\frac{1}{2.3.4}ax^4$: so that we have $\frac{1}{2}ax^2 - \frac{1}{2.3.4}ax^4$ for the corrected displacement: and this correction will, in a similar manner, afford a second of $\frac{1}{2..6}ax^6$; so that the true

displacement becomes $\frac{1}{2}ax^2 - \frac{1}{2..4}ax^4 + \frac{1}{2..6}ax^6 - \frac{1}{2..8}ax^8 - \ldots$, which, as is well known, is equal to $a - a\cos. x$: and the diminution of the velocity $ax - \frac{1}{2 \times 3}ax^3 + \frac{1}{2..5}ax^5 - .. = a\sin. x$; which will, of course, vanish when $x = 180°$; so that the body will be at rest at the expiration of the corresponding time of a complete vibration in one direction. And a similar mode of calculation may be applied to the case of a simple pendulum, with a resistance varying as the square of the velocity, except that here the variation of the resistance at each step makes the process more complicated.*

THEOREM D. If the resistance be proportional to the square of the velocity, a pendulum, of which the point of suspension performs vibrations composed of two regular vibrations, may have its greatest excursions a little after the greatest excursions of the point of suspension when its vibrations are inverted, and a little before them when they are direct, provided that the slower vibrations be the larger.

In order to express the resistance as correctly as possible in this case by a series of multiple arcs, it would be necessary to have a great variety of terms, some approaching in their periods to the primitive vibrations, others triple and quintuple of these: but for the present purpose these greater multiples may be safely omitted, taking care only that the omission do not affect the determination of the coefficients of the rest. The general methods of obtaining a series in the terms of sines and cosines of multiple arcs fail here, as before, on account of the positive terms resulting from the squares of negative quantities, where the conditions of the problem require that they should be negative, and it is necessary to employ approximations obtained from the results of individual substitutions. For this purpose a series of five or six terms has been tried in various ways without success: and the most convenient form which has been discovered consists of three only, two isochronous with the primitive

* The problems considered in this and the following theorem are treated in a much more complete and scientific form, by the aid of a more modern and refined analysis, in section iii. of the article which follows, No. LV. The methods adopted here make a bold and, the circumstances being considered, a tolerably successful inroad upon the solution of a problem of great difficulty by means which are apparently hardly sufficient for the purpose.—*Note by the Editor.*

vibrations, and the third having a recurrence less frequent by one time in the common period than the slowest of these: then the coincidence being established at the time of the greatest and least excursions, and at the transit of the vertical line nearest to the middle of the intermediate time, a mean value of the coefficients may be obtained, which nowhere differs very materially from the truth; although, if we desire to make the coincidence more perfect in any given part of the period, we may do it by altering the values of the coefficients a little; and by these means we may obtain a correction of the approximation, sufficiently near to the truth. We may also suppose the actual compound vibrations to preserve their regularity without any material deviation, following the same law as if the resistance were either inconsiderable, or varied simply as the velocity; and we may make the proportion of the greatest to the least actual vibration that of $m+1$ to $m-1$; then calling the periodical time of the greater primitive vibration (☽) t, that of the lesser (☉) being unity, and x being the arc corresponding to the time in the latter, beginning with the perfect coincidence in the vertical line, the distance from that line at any subsequent time will be expressed by $\sin. x+m \sin. \frac{x}{t}$; and the velocity by $\cos. x + \frac{m}{t} \cos. \frac{x}{t}$, creating a resistance which may be called $r \left(\cos. x + \frac{m}{t} \cos. \frac{x}{t}\right)^2$, which has already produced a displacement determinable as in the former proposition, whence we may obtain from the true place the place in which the body would have been found if there had been no resistance. In order to facilitate the computation, we may assume particular values of m and t, making the one 3, and the other $1\frac{1}{29}$; and then determine the coefficients of the formula $a \cos. x + b \cos. \frac{29x}{30} + c \cos. \frac{28}{30} \cos. x = \left(\cos. x + 2{\cdot}9 \cos. \frac{29x}{30}\right)^2$, so as to obtain as correct a coincidence as possible of the magnitude and of the period of the joint vibrations at the time more immediately to be considered. Now it is easily shown, from the well-known properties of compound vibrations, as applied to the intervals of successive spring and neap tides, that the interval between two

of the greatest vibrations will be expressed very nearly by $\frac{m+1}{m+t}\, t\ 360°$, and the interval between two of the smallest by $\frac{m-1}{m-t}\, t\ 360°$, provided that the periods differ but little from each other: and from these formulas we must determine the proportions of the coefficients a and c; for b must always be $1 + 2{\cdot}9^2 = 9{\cdot}41$, in order that the equation may hold good for the greatest and least vibrations; consequently $a + c = 5{\cdot}8$. We may first allow $\frac{9{\cdot}41}{3}$ to a, in order to form with b a result similar to the true compound vibration, and the remainder 2·663 must again be distributed between a and c in such a proportion that the interval of the greatest vibrations may be $\frac{40}{35}\ 360°$, and m must be so determined for this purpose that $\frac{m+1}{m+\frac{30}{28}} \cdot \frac{30}{28}$ shall be equal to $\frac{40}{35}$, whence $m = \frac{3}{5}$, and for this part of a, $'a : c = 1 : \frac{3}{5}$, $'a : a + c = 1 : \frac{8}{5}$, and $'a = \frac{5}{8} \,.\, 2{\cdot}663 = 1{\cdot}664$, $a = 4{\cdot}80$, and $c = 1$, so that the velocity becomes $4{\cdot}8 \cos. x + 9{\cdot}41 \cos. \frac{29x}{30} + \cos. \frac{28x}{30}$: and for the interval of the least vibrations, $\frac{m+1}{m+\frac{30}{28}} \,.\, \frac{30}{28} = \frac{20}{19}$, and the whole is found $3{\cdot}8 \cos. x + 9{\cdot}41 \cos. \frac{29}{30} x + 2 \cos. \frac{28}{30} x$; and for a mean value of the coefficients, $4{\cdot}3 \cos. x + 9{\cdot}41 \cos. \frac{29}{30} x + 1{\cdot}5 \cos. \frac{28}{30} x$.

If now we denote the ratio of the square of the time of the most frequent vibration ☉ to that of the square of the time of the spontaneous vibration of the pendulum (♒) by the ratio of n to 1, the corresponding displacement will be to the distance expressive of the force as $\frac{n}{n-1}$ to 1, and the term $4{\cdot}8 \cos. x$ will exhibit a displacement of $\frac{4{\cdot}8n}{n-1} \cos. x$: and in the other terms, substituting, for n, $(\frac{30}{29})^2 n$ and $(\frac{30}{28})^2 n$ respectively, we have $\frac{1{\cdot}07n}{1{\cdot}07n-1}$ and $\frac{1{\cdot}145n}{1{\cdot}145n-1}$, or $\frac{n}{n-{\cdot}9344}$, and $\frac{n}{n-{\cdot}871}$ for multipliers; and the distance thus determined shows the place in which the body would have been if there had been no resistance, which is before the true place when the multiplier is positive, and behind when it is negative: the distance of this

virtual place therefore becomes sin. x + 3 sin. $\frac{29x}{30}$, sin. x + 3 sin. $\frac{29x}{30} + rn\left(\frac{4\cdot8}{n-1}\cos x + \frac{9\cdot41}{n-\cdot9344}\cos.\frac{29x}{30} + \frac{1}{n-\cdot871}\cos.\frac{28x}{30}\right)$, and the velocity cos. $x + 2\cdot9\cos.\frac{29x}{30} - rn\left(\frac{4\ 8}{n-1}\sin. x + \frac{29}{30}\cdot\frac{9\cdot41}{n-\cdot9344}\cos.\frac{29x}{30} + \frac{28}{30}\cdot\frac{1}{n-\cdot871}\cos.\frac{28x}{30}\right)$. Here it is obvious that as n approaches to 1, to ·9344, or to ·871, the value of the corresponding term increases without limit, and the period of the resistance may approach to that of the slower vibration, or may even exceed it, in very particular circumstances: and if these periods were equal, the effect would be the same as if the whole resistance were attached to the slower vibration, which would obviously be such as is stated in the theorem. But for a more particular illustration, we may take $n = \frac{2}{3}$, and $r = \frac{1}{20}$: the distance of the virtual place will then become sin. x + 3 sin. $\frac{29x}{30}$ − ·48 cos. x − 1·103 cos. $\frac{29x}{30}$ − ·163 cos. $\frac{28}{30}x$: and by substituting in this formula a number of different values for x, we find, when x = 118°, −252°, and −623°, maxima amounting to 4·353, 4·367, and 4·342 respectively; and, employing the other values of a and c, a maximum of 2·055 when $x = 15 \times 360° - 280°$. Here it is obvious that the maximum for the virtual place is anterior to the true maximum, the excursion 4·367 being considerably greater than 4·353, which is nearest to the true maximum; or, in other words, the true maximum happens a little after the perfect conjunction of the forces which occasion it, which, if there were no resistance, would coincide with the maximum of the excursions.

THEOREM E. The disturbing force of a distant attractive body, urging a particle of a fluid in the direction of the surface of a sphere, varies as the sine of twice the altitude of the body.—See *supra*, No. XLIV., p. 121.

THEOREM F. The inclination of the surface of a spheroid, slightly elliptical, to that of the inscribed sphere, varies as the sine of twice the distance from the circle of contact; and a particle resting on any part of it without friction may be held

in equilibrium by the attraction of a distant body. —See *supra*, No. XLIV., p. 121.

Corollary. Hence it may be calculated, neglecting the density of the sea, that the primitive solar tide would be ·807. and the lunar 2·0166 feet, supposing the lunar disturbing force to the solar as 5 to 2.

THEOREM G. The disturbing attraction of the thin shell, contained between a spheroidical surface and its inscribed sphere, varies in the same proportion as the inclination of the surface, and is to the relative force of gravity, depending on that inclination, as 3 times the density of the shell to 5 times that of the sphere.—See *supra*, No. XLIV., p. 124.

Corollary. Hence the ellipticity must be to that which would take place if the density n of the sphere were infinite, as 1 to $1-\frac{3}{5n}$; or, in the case of $n=5\frac{1}{2}$, nearly as 8 to 9, giving for the solar tide ·91, and for the lunar 2·263: if $n=5$, the heights are ·92 and 2·291 respectively; if $n=1$, 2·024 and 5·042.

Scholium. The direct attraction, determining the length of the pendulum in different latitudes, may be calculated in a manner nearly similar.—See *supra*, No. XLV., p. 127.

THEOREM H. When the horizontal surface of a liquid is elevated or depressed a little at a given point, the effect will be propagated in the manner of a wave, with a velocity equal to that of a heavy body which has fallen through a space equal to half the depth of the fluid, the form of the wave remaining similar to that of the original elevation or depression.*

Scholium. In calculating this velocity, it would probably be more correct to diminish the multiplier about $\frac{1}{10}$ or $\frac{1}{8}$, as is found to be necessary for determining the velocity of the motions of fluids in most other cases.†

THEOREM I. A wave of a symmetrical form, with a depression equal and similar to its elevation, striking against a solid vertical obstacle, will be reflected so as to cause a part of the surface, at the distance of one-fourth of its breadth, to remain

* See supra, No. XLVII., p. 141. † See supra, No. XXIV.

at rest; and if there be another opposite obstacle at twice that distance, there may be a perpetual vibration between the surfaces, the middle point having no vertical motion.—*See* 'Young's Natural Philosophy,' I. 289, 777.

Thus the vibrations of the water supposed to be contained in a canal in the situation of the equator, and 90° in length, would be synchronous with the passage of a wave 180° in breadth over any point of a canal of the same depth: and the elevation and depression of a spheroid, compared with the mean height, exhibits a symmetrical wave in the sense of the proposition.

THEOREM K. The oscillations of the sea and of lakes, constituting the tides, are subject to laws exactly similar to those of pendulums capable of performing vibrations in the same time, and suspended from points which are subjected to compound regular vibrations of which the constituent periods are completed in half a lunar and half a solar day.

Supposing the surface of the sea to remain at rest, each point of it will become alternately elevated and depressed in comparison with the situation in which it would remain in equilibrium, its distance from this situation varying according to the regular law of the pendulum (see Theorem F): and it will be actuated by forces indirectly dependent on, and proportional to this distance, so that it may be compared to a pendulum remaining at rest in the vertical line about which its point of suspension vibrates, and will consequently follow the motion of the temporary horizon in the same manner as the pendulum follows the vibration of its point of suspension, either with a direct or retrograde motion according to circumstances: the operation of the forces concerned being perfectly analogous, whether we consider the simple hydrostatic pressure depending on the elevation, or the horizontal pressure derived from the inclination of the surface, or the differential force immediately producing elevation and depression, depending on the variation of the horizontal pressure, and proportional to the curvature of the surface. We have only to determine the time of spontaneous oscillation ($=$) either in the open sea, or in any confined chan-

nel or lake of known dimensions, and we may thence immediately infer the magnitude of the solar or lunar tide, supposing the resistance inconsiderable; and supposing the resistance given, we may obtain by approximation a sufficiently correct idea of its effects.

Corollary 1. Neglecting, in the first place, the resistance, we may suppose a lake or sea to be contained between opposite coasts in the direction of the meridian, and call its breadth in the direction of the equator b, and its depth d, both in miles; then the time required for the complete oscillation of such a lake will be $\frac{b}{140\sqrt{d}}$ in hours: and the square of this time will be to the square of half a solar day (⊙) as $\frac{b^2}{19600d}$ to 144, or as b^2 to $2830000d$, :: 1 : n (Pr. A. Sch. 2.)* and $n = 2830\,000\ d : b^2$, and $\frac{n}{n-1} = \frac{2830000d}{2830000d-b^2}$, which is the multiplier for determining the excursion of the pendulum from that of the point of suspension, or the true height of the tide from the variation of the form of equilibrium: so that if b be considered as a circular arc, the height at the eastern and western shores will be sin. $\frac{1}{2}\ b\ \frac{2830000d}{2830000d-bb} \cdot h$, h being the whole height of the primitive solar variation; and in the same manner taking half a lunar instead of half a solar day, we have sin. $\frac{1}{2}\ b\ \frac{3030000d}{3030000d-bb}$ h, for the lunar tide, h being the primitive lunar variation: and for a lake of 90° in breadth, where $b = 6216$, or for the open ocean, the heights become $\frac{d}{d-14}\ h$, and $\frac{d}{d-13}\ h$ respectively. It would, however, probably be more correct to make the numbers 14 and 13 somewhat larger, on account of the deficiency of velocity observable in almost all the motions of fluids.

Corollary 2. In this calculation we neglect the attraction of the parts of the sea already elevated or depressed, so that it would only be strictly accurate if the density of the sea were absolutely inconsiderable, and h were ·8 or 2 feet. But if the earth consisted wholly of a substance not more dense than water, the force tending to destroy the level of its surface would

* Supra, p. 264.

be only $\frac{2}{5}$ as great as the disturbing force which would act at the same point if the body had assumed the form of equilibrium, since $\frac{3}{5}$ of the force would be the effect of the attraction of the parts actually elevated (Theorem G): and the ratio of the forces would be the same in every part of the vibration; so that the time of a spontaneous oscillation would be increased in the subduplicate ratio of the diminution of the force, and the value of n diminished in the simple ratio. And if we suppose the density of the earth to be about $5\frac{1}{2}$ times as great as that of the sea, the value of n becomes reduced to $\frac{49}{55}\,n$, and we find for the solar tide of the open sea $\frac{\cdot 91d}{d-15\cdot 7}$, and for the lunar $\frac{2\cdot 263d}{d-14\cdot 6}$ $= q$; and having the actual height q, $d = \frac{15\cdot 7q}{q-\cdot 91}$, or $\frac{14\cdot 6q}{q-2\cdot 263}$; the depth 15·7 and 14·6 miles being the smallest at which the tides could be direct: supposing the sea shallower, they would be inverted, the passage of the luminary over the meridian corresponding with the time of low water.

Corollary 3. We may form a coarse estimate of the effect of resistance on the height and time of the tides of a given sea by considering the case of a simple oscillation subjected to a resistance proportional to the velocity. Supposing the retardation or acceleration of a lunar tide to amount to one lunar hour, the arc of the circle appropriate to the vibration becoming 30°, the cosine of this arc will be ·866, and the height will be ·866. $\frac{2\cdot 263d}{d-14\cdot 6}$, (Th. B) for the open sea, and $d = \frac{14\cdot 6q}{q-1\cdot 96}$: Thus if the height were 2 or -2, d would be 7·30 or 7·37, while the formula $\frac{14\cdot 6q}{q-2\cdot 263}$, independent of resistance, would give only 6·45, a negative value of d being impossible. If h were -3, with this resistance, d would be 8·83. The sine of 30° being ·5, the resistance, when greatest, would be equal to half the greatest accelerating force.

Corollary 4. If the bottom of the sea were perfectly smooth and horizontal, we might form some idea of the resistance opposed to the tides from the phenomena of rivers and pipes; but on account of the great irregularity of form, we can only infer that the resistance must be incomparably greater than that

which is thus determined. The horizontal velocity is most readily deduced from the effect of the inclination, which generates a force varying according to the law of the pendulum, and producing, therefore, a velocity, which, when greatest, is to that which would have been produced by the whole force uniformly continued for the same time, as the radius is to one fourth of the circumference: the sine of the inclination, which expresses the force, is also to the whole height divided by the breadth, as one fourth of the circumference to the radius: so that the greatest velocity becomes precisely equal to that which would be produced in the same time by a uniform force, expressed by the height of the tide divided by the breadth: and for the solar tide in the open sea, we have a force expressed by the sine $\frac{q}{6216 \times 5280}$ operating for three hours, which is equivalent to the force of gravity operating for $\frac{3q}{6216 \times 5280}$, and will generate a velocity of $\frac{345600q}{6216 \times 5280} = \frac{45q}{4279} = \frac{q}{95 \cdot 1}$ in a second, or, if q be supposed equal to 1 foot, about $\frac{1}{8}$ of an inch. Now it appears from the experiments of Dubuat and others (Phil. Trans. 1808)*, that the resistance may be expressed in inches of pressure by the formula $f = a \frac{l}{d} v^2 + 2 c \frac{l}{d} v$, where, for considerable depths, $a = \cdot 0000413$, and $c = \cdot 00009$, or perhaps $\cdot 0001$, d being four times the depth; but instead of having f as a measure of the height corresponding to the resistance, we may determine the equivalent inclination by finding $\frac{f}{l}$, which will be $\frac{av^2 + 2cv}{d}$; and this we are to compare with the greatest force tending to produce the horizontal motion, or $\frac{1 \cdot 5708q}{6216 \times 5280}$. But since $v = \frac{4147200q}{6216 \times 5280}$, $\frac{av^2 + 2cv}{d}$: $\frac{1 \cdot 5708q}{6216 \times 5280} = \frac{av + 2c}{d} : \frac{1 \cdot 5708}{4147200}$, and the greatest resistance will be to the greatest propelling force as $\frac{4147200}{1 \cdot 5708}(av + 2c)$ to d, or as $35v + 126$ to the depth in inches, that is, as $\frac{q}{2 \cdot 7} + 10\frac{1}{2}$ feet to the depth. Hence it appears that if this calculation

* Supra, No. XXIV., vol. i.

were sufficient to determine the magnitude of the resistance, that part of it which varies as the square of the velocity would be small in comparison with the part which varies as the velocity, not only for a tide of one foot, but even for one of ten feet in height; and that both parts would become almost insensible in a sea of considerable depth. In fact, however, the observations have been made under circumstances so widely different, that no valid conclusions can be formed from them with respect to depths so great and velocities so small, even if we could disregard the irregularities of the bottom of the sea, which, by the eddies and other deviations depending on them, must create a much greater resistance than the calculation indicates; and this resistance, from the nature of the centrifugal forces concerned in it, is much more likely to vary as the square of the velocity than as the velocity simply. If we employed Dubuat's original formula $v = (\sqrt{r} - \cdot 1)\left(\frac{297}{\sqrt{b} - \mathrm{HL}\sqrt{(b+1.6)}} - \cdot 3\right)$, we might infer that their resistance or adhesion would annihilate the velocity when $\cdot 3(\sqrt{b} - \mathrm{HL}\sqrt{(b+1 \cdot 6)})$ became equal to 297, b being the cosecant of the inclination, or here $\frac{6216 \times 5280}{1 \cdot 5708\, q}$; so that if $q = 1$, $\cdot 3\,(\sqrt{b} - \mathrm{HL}\,\sqrt{(b+1 \cdot 6)}) = 1369$: consequently nothing can be inferred from the calculation, except that Dubuat's formula is totally inapplicable to the case; perhaps, however, the extravagant resistance, which is indicated by it, may be admitted as a conjectural argument, to shew that the resistance, even in a sea of a form perfectly regular, would probably be greater than is inferred from the formula for pipes and rivers, published in the Phil. Trans.

Corollary 5. We are next to inquire what would be the effect of a considerable resistance, varying as the square of the velocity, on the compound tide, produced by the combination of the lunar and solar forces: and the calculations in Theorem D will serve to illustrate this case as it is found in nature. The first remarkable consequence of such a resistance is the alteration of the comparative magnitudes of the forces concerned: the extent of the oscillations being diminished by the resistance, the diminution will be greater where the resistance is greater for a given velocity, and the spring tides will bear a smaller propor-

tion to the neap than if there were no resistance, so that the apparent inequality of the solar and lunar forces will be greater than their true inequality. We must, however, remember in making this calculation, that the proportion of the tides is by no means precisely the same with that of the disturbing forces of the luminaries, but may differ from it more or less on account of the difference of the periods, according to the depth of the ocean, and the form and magnitude of the seas and lakes concerned. For example, taking $n = \frac{2}{3}$ and $r = \frac{1}{20}$, or since $n = \frac{d}{15 \cdot 7}$, $d = 10\frac{1}{2}$ miles, the greatest resistance being supposed for the solar tide equal to $\frac{1}{20}$ of the greatest propelling force; it appears that under these circumstances, if the true spring and neap tides are generally as 2 to 1, which seems to be very nearly their true proportion, the tides which would happen if the resistance were annihilated would be in the proportion of 4·367 to 2·055, and the primitive forces exciting these tides, instead of 6·422 and 2·312, would be $6{\cdot}422\,\frac{n - {\cdot}9344}{n}$ and $2{\cdot}312\,\frac{n-1}{n}$, or in the proportion of 5·158 to 2·312, or 2·208 to 1. It is obvious therefore, that without a more correct knowledge of the depth of the sea, and the resistances to its motion, than we possess, it is impossible to form any accurate estimate of the proportion of the solar and lunar forces from the tides, even if we suppose our observations to be exempt from the operation of any of those local causes which have been described, as likely to influence this proportion: in fact it is not improbable that the irregularities of the form of the seas are so great as to set at defiance all calculation, even if they were ascertained.

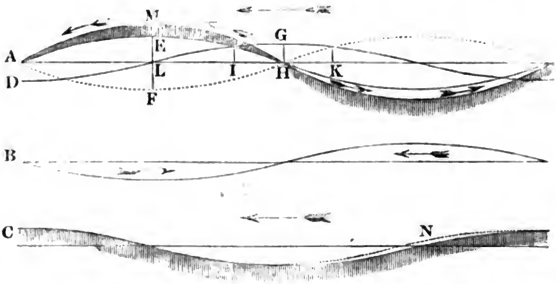
Corollary 6. The time of high water is also subjected to various modifications, according to the resistances concerned. It is easy to see, that a resistance of any kind will produce a retardation of the direct, and an acceleration of the inverted tides (Cor. 3); but the law of a resistance varying as the square of the velocity produces two remarkable consequences with respect to the time of high water: first, that the spring tides will be retarded or accelerated more than the neap tides; and secondly, that the highest tides will not be precisely at the

syzygies, but may be before or after them, according to circumstances. The first of these consequences has not been sufficiently established by observation, although it has been remarked in general, that high tides happen earlier than lower ones, other things being equal. But in many of the harbours in which the most accurate observations have been made, the time of high water may perhaps be somewhat modified by the different resistances opposed to tides of different magnitudes in their passage from the seas in which they originate. The second circumstance is observed in a greater degree than can be well explained from the present state of the calculation. It is not easy to suppose the conditions more favourable to the retardation of the spring tides, than they have been assumed in the case stated in Theorem D; and the maximum is here scarcely at the distance of a single tide from the conjunction, the second excursion being somewhat smaller than the excursion immediately preceding the conjunction, nor is it probable that the imperfection of the mode of calculation is so great as to afford a result very materially different from the truth. It must, therefore, remain, for the present, as a difficulty to be solved by future investigations, that in many ports not far remote from the open sea, to which the tides can by no means be, as Laplace seems to suppose, a day and a half in travelling, the third tide after the syzygy is, in general, the highest; if, indeed, this fact should be confirmed in all its extent by observations made in a greater variety of situations than those which have hitherto been recorded. The advance of the spring tides at the solstices, which Laplace has adduced as an illustration of the dependence of the tides on the state of the luminaries a day and a half before, is certainly favourable to his opinion. But this circumstance is not sufficiently established by continued observation, to counterbalance the incompatibility of so slow a progress of the tides from the open ocean with the well-known times of high water at the different ports. There is also some difficulty in explaining the occurrence of high water at St. Helena 2¼ hours, and at Belleisle 2¼ after the moon's transit, in these situations, which seem to be as little remote as possible from the source of the tides of the Atlantic. The direct tides on the eastern coasts

of a sea like the Atlantic ought to happen about an hour before, and the inverted almost five hours after the transit; an acceleration of 2½ hours seems to be greater than would be expected according to any probable estimate of the magnitude of the resistance. The simplest solution of this difficulty seems to be to suppose the deepest parts of the Atlantic much narrower than the whole of that ocean, so as to cause the simple inverted tide to happen considerably before the fifth lunar hour; unless we choose to consider the principal part of the phenomena of our tides as dependent on the affections of distant seas, so as to occur at times very different from those of the primitive variations of the Atlantic. The tides on the western coasts agree very well with either supposition. The slight difference of the ascent and descent of the tide, remarked by Mr. Laplace in the observations at Brest, may be explained by a comparison with the form of a common wave, which, where the water is shallow, is always steepest before. This circumstance arises from the greater velocity with which the upper parts of the wave advance, where the difference of the depths becomes considerable (Phil. Trans. 1808); and it is, perhaps, somewhat increased by the resistance of the bottom. Where the tide travels far in shallow channels, its irregularity of inclination increases more and more; for instance, in the Severn, it assumes almost the appearance of a steep bank. (See Nich. Journ., vol. XVIII., 1807, p. 118.)

Scholium 1. As a confirmation of the conclusions which have been deduced from the analogy of the oscillations of the fluids with the vibrations of the pendulum, we may obtain similar results from the immediate consideration of the progress of the tide in an open ocean following the luminary like a widely extended wave. If the actual height of the tide be called q, its virtual height, with respect to the form which would afford a momentary equilibrium, will be $q \pm h$, and the propelling force will be proportional to this height: now the natural velocity of the wave, $m\surd(\frac{1}{2}d)$, is generated by its exposure to a force proportional to its simple height for a time proportional to that which it occupies in passing over its own breadth b, that is for the time $\frac{b}{m\surd(\frac{1}{2}d)}$; and if it be exposed to a force greater

or less in the ratio of $q \pm h$ to q, for the time $\frac{b}{n}$, where n is the velocity of rotation, its velocity will become $m \sqrt{(\frac{1}{2}d)} \frac{q \pm h}{q} \frac{b}{n}$. $\frac{m\sqrt{(\frac{1}{2}d)}}{b} = m^2 . \frac{1}{2}d \cdot \frac{q \pm h}{nq}$, which must be equal to n, and $n^2 q = m^2 \frac{1}{2}d\ (q \pm h)$, and putting $n^2 = m^2 . \frac{1}{2}r$, $rq = d\ (q \pm h)$ and $q = \frac{\pm dh}{r - d}$, and $d = \frac{rq}{q \pm h}$, precisely as already demonstrated; r being the depth, affording a velocity equal to the velocity of rotation. And supposing, in the next place, such a wave to be liable to a resistance proportional to the velocity, we may illustrate the effect of the resistance by comparing it with that of another wave, which we may call its representative, combined with the original wave, and altering the inclination of its surface to the horizon. Thus, if the ordinates of the curve A



represent the elevation and the horizontal velocity of the tide, those of the curve B will exhibit the force derived immediately from the resistance, which will be the same as would be produced by the combination of the wave C with the original wave, or as if the momentary position of the virtual horizon were altered from its natural state to the form D, which is the reverse of C; and this form D must be combined with the virtual variation of the horizon corresponding to the primitive tide, in such a manner as to produce a result agreeing in its position with the actual tide, and such as is represented by the curve AE for the direct, or AF for the inverted tide: and this will ob-

viously happen if the primitive variation be represented by the space included between AE or AF and DC, the resistance GH being proportional to the sine of the displacement HI or HK, and the height of the result LE or LF, on which the true height LM immediately depends, to its cosine: the true maximum at M following that of the space AEL, and preceding that of FGH, as has already been shown by a different method.

It may be remarked, that since the resistance probably varies with the depth, the retrograde motion of the waters will be more impeded than the direct, and a very slow, although perhaps an imperceptible, current from east to west will thus be established, its velocity being always less than that which is sufficient to produce an equality of resistance in the different directions.

Scholium 2. It has been observed, in Corollary 6, that the time of high water may perhaps be modified by the resistances opposed to the passage of the tides from the open ocean into the ports at which they have been observed. And indeed, without a mature consideration of the subject, it would have been natural to speak with less hesitation of the effect of resistance in retarding the propagation of an undulation through a fluid. In reality, however, this retardation appears to be very inconsiderable, if it exists at all, at least where the height of the wave is moderate in proportion to that of the fluid. We may suppose AM to be the figure of an undulation advancing simply in a channel of a given depth, with a resistance proportional to the velocity, which may again be represented by the virtual elevation of the wave C, which must always accompany the original undulation AM in its progress, and must, therefore, constantly tend to produce the same motions in the particles of the fluid as a real wave of the same magnitude; for the figure of the surface, and the velocity with which that figure successively changes or advances, are the only causes which furnish the immediate forces concerned in producing the elementary motions constituting the oscillations. Now, while the imaginary wave C advances a little into the situation N, it is obvious that, in consequence of the action of the forces which it represents, the surface must be elevated at N, where the surface of AM is depressed, and depressed in the half of the undulation

next to C, where AM is elevated, so as always to diminish the magnitude of the original undulation, without affecting its velocity, as it would do if the curve C crossed its absciss in any other points than those which correspond to the greatest ordinates of AM. And the rate of diminution will be such, that if it continued uniform, the wave AM would be lowered at M during the time that it passes over one-fourth of its whole breadth AL, by a quantity which is to the greatest ordinate of C, the representative of the resistance, as the semi-circumference of a circle to its diameter; but since the resistance would vary with the height of the wave, the actual diminution would be expressed by a logarithmic quantity. Thus, if the greatest resistance be to the greatest propelling force as r to 1, the fluxion, or rather variation, of the height q will be to that of the absciss x as $90° \times qr$ to $90°$, and $-\dot{q} = rq\dot{x}$, $-\frac{\dot{q}}{q} = r\dot{x}$, and H. L. $q = C - rx$, or, calling the primitive value of q unity, $q = e^{-rx}$, and $\frac{1}{q} = e^{rx}$.

E. F. G. H.

London, June, 1811.

No. LV.

TIDES.

From the Supplement to the Encyclopædia Britannica.

Written in 1823.

The investigation of the phenomena of the tides has been justly considered as uniting some of the greatest difficulties that occur in the various departments of natural philosophy and astronomy. It implies, first, a knowledge of the laws of gravitation, concerned in the determination of the forces immediately acting on the sea, and of the periods and distances of the celestial bodies, which modify the magnitudes and combinations of these forces; and secondly, of the hydraulic theories of the resistances of fluids, and of the motions of waves and undulations of all kinds, and of the theoretical determination of the form and density of the earth, as well as of the geographical observation of the breadth and depth of the seas and lakes which occupy a part of its surface; so that the whole subject affords abundant scope for the exercise of mathematical skill, and still more for the employment of that invention and contrivance which enables its possessor to supersede the necessity of prolix computations, wherever they can be avoided.

The history of the theory of the tides is naturally divided into several periods in which its different departments have been progressively cultivated. The ancients, from the times of Posidonius and Pytheas, and the moderns before Newton, were contented with observing the general dependence of the tides on the moon, as following her transit at an interval of about two hours, and their alternate increase and decrease not only every fortnight, but also in the lunar period of about eight years; the second step consisted in the determination of the

magnitude and direction of the solar and lunar forces, by which the general effects of the tides were shown, in the *Principia*, to be the necessary consequences of these forces: the third great point was the demonstration of Maclaurin, that the form of an elliptic spheroid affords an equilibrium under the action of the disturbing forces concerned; while the further contemporary illustrations of the subject by Euler and Bernoulli, though they afforded some useful details, involved no new principle that can be put in competition with Maclaurin's demonstration: the fourth important step was made by Laplace, who separated the consideration of the *form*, affording mere equilibrium, from that of the *motion* occasioned by the continual change of that form, while former theorists had taken it for granted that the surface of the sea very speedily assumed the figure of a fluid actuated by similar forces, but remaining perfectly at rest, or assuming instantly the form in question. Laplace's computation is however limited to the case of an imaginary ocean, of a certain variable depth, assumed for the convenience of calculation, rather than for any other reason. Dr. Thomas Young has extended Laplace's mode of considering the phenomena to the more general case of an ocean covering a part only of the earth's surface, and more or less irregular in its form; he has also attempted to comprehend in his calculations the precise effects of hydraulic friction on the times and magnitudes of the tides.* As far as the resistance may be supposed to vary in the simple ratio of the velocity, Dr. Young's theory is sufficiently complete, and explains several of the peculiarities which are otherwise paradoxical in their appearance; but there still remains a difficulty to be combated with respect to the effects of a resistance proportional to the square of the velocity, and this, it is hoped, will be in great measure removed in the present article, which, however, from the space that is allotted to it, must be considered rather as a supplementary fragment than as a complete treatise. It will be divided into four sections: the first relating to the contemporaneous progrses of the tides through the different seas and oceans, as collected from observation only; the second, to the

* Supra, p. 281.

magnitude of the disturbing forces tending to change the form of the surface of the earth and sea; the third to the theory of compound vibrations with resistance; and the fourth to the application of this theory to the progress and successive magnitudes of the tides, as observable at any one port.

SECT. I.—*Of the Progress of Contemporary Tides, as inferred from the times of High Water in different Ports.*

The least theoretical consideration, relating to the tides, is that of their progress through the different parts of the ocean, and of its dependent seas. The analysis of these ought to be very completely attainable from direct observation, if the time of high water had been accurately observed at a sufficient number of ports throughout the world; and, on the other hand, if the earth were covered in all parts with a fluid of great and nearly uniform depth, the tides of this fluid would be so regular, that a very few observations would be sufficient to enable us to deduce the whole of the phenomena from theory, and to trace the great waves, which would follow the sun and moon round the globe, so as to make its circuit in a day, without any material deviation from uniformity of motion and succession. Having collected, for the actual state of the sea and continents, an abundant store of accurate observations of the precise time of high water with regard to the sun and moon, for every part of the surface, and having arranged them in a table according to the order of their occurrence, as expressed in the time of any one meridian, we might then suppose lines to be drawn on a terrestrial globe, through all the places of observation, in the same order; and these lines would indicate, supposing the places to be sufficiently numerous, so as to furnish a series of tides very nearly contemporary, the directions of the great waves, to which that of the progress of the tides in succession must be perpendicular. If, however, we actually make such an attempt, we shall soon find how utterly inadequate the observations that have been recorded are, for the purpose of tracing the forms of the lines of contemporary high water with accuracy or with certainty, although they are abundantly sufficient to show the impossibility of deducing the time of high

water at any given place from the Newtonian hypothesis, or even from that of Laplace, without some direct observation. It might at least be supposed very easy to enumerate the existing observations, scanty as they may be, in a correct order; but there are a number of instances in which it is wholly uncertain whether the time observed at a given port relates to the tide of the same morning in the open ocean, or to that of the preceding evening; this inconvenience may, however, in some measure be remedied, by inserting such places in two different parts of the table, at the distance of 12½ hours from each other.* The following Table is the result of the best approximation that could be obtained in this manner, the principal hour lines having been partially traced on a map of the world, in order to afford some little direction to the correct insertion of the times of high water without the material error of half a day. The three great natural divisions of the Atlantic, the Pacific, and the Indian Oceans, are distinguished into different columns, and some of the places, which seem to form separate systems of tides, are still further subdivided in a similar manner; but this arrangement may easily be neglected by those who are inclined to question its propriety. The principal authorities are the 'Requisite Tables;' Lalande's 'Treatise,' in the fourth volume of his 'Astronomy;' a manuscript 'Hydrographical Memoir' by Mr. Henry Foster, Lieut. R.N., of his Majesty's ship Conway, Capt. Basil Hall; and 'A Series of Observations made at the Island of Ascension in 1820,' by Captain Campbell, R.N., obligingly communicated by Captain Hall to the author of this Article.

TABLE OF THE TIME OF HIGH WATER AT THE FULL AND CHANGE OF THE MOON; REDUCED TO THE MERIDIAN OF GREENWICH.

Atlantic. Pacific. Indian.	Longitude.	H. W. Gr. T.
	H. M.	H. M.
S. Georgia	1.26 W.	1. 0
Cape of Good Hope	1.14 E.	1.30
St. Helena	0.23 W.	2.38
Goree Island. (See below.)		
Cape Corse	0. 7	3.35
Canaries. (See below.)		

* This should be 12h. 18m. See below, p. 347.—*Note by the Editor.*

Atlantic.	Pacific.	Indian.	Longitude.	H. W. Gr. T.
			H. M.	H. M.
Rio Janeiro			2.53	4.58
I. Martin Vaz			1.56	5.41
I. Ascension			0.57	5.42 C.
Christmas Sound			4.46	7.10
St. Jago			1.34	7.34 or 12.34 Lal.
Pt. Désiré			4.20	8.35
	St. Helena, S. A.		4.40	8.40
	Quibo		5.29	8.59
Sierra Leone			0.53	9. 8
	Easter I.		7.19	9.19
St. Julian's			4.35	9.20
Marañon, Mouth			3.20	9.20
St. John's, Newf.			3.30	9.20
Guadaloupe			4. 7	10. 7
	Panamá		5.21	10.21 Lal.
Tortugas			4.51	10.51
Cape Blanco			8.16	10.54
Bermudas			4.14	11.14
Martinique			4. 5	11.35
	Guayaquil		5.17	11.17
Senegal			1. 6	11.36
	Callao		5. 8	11.38
Halifax			4.14	11.14
	Marquesas		9.16 to 9.17	11.46 to 12.16
Quebec			4.44	12.14
Cape La Hogue			0. 8	12.38
	Panamá		5.21	12.41 F.
Gibraltar			0.20	12 50
	Tahoga, Pan. Bay		5.22	13.10 F.
Funchal			1. 8	13.42
Portobello			5.19	13.19
[S. Georgia			2.26	13.26 ?]
Cape Bojador			0.58	13.28
Funchal			1. 8	13.42
Churchill R. / P. of Wales's Fort			6.17	13.37
Terceira			1.49	13.34 or 14. 4
New York			4.57	13.57
Cape Henlopen, Virg.			5. 1	14. 1
Cadiz			0.25	14. 5 uncertain.
	Karakakooa B		10.24	14. 9
Virgin Cape, Pat.			4.32	14.32
	Valparaiso		4.49	14.50
Cape Charles			4 57	14.57
Goree Island			1.10	15.10
York Fort			6. 9	15.19
Lisbon			0.37	15.22
Nantes, Rhé			0. 6	15.36
	Tanna		11.19	15.41
Brest			0 18	16. 3
Bayonne			0. 6	16. 6
C. St. Vincent			0.36	16. 6
Corunna			0.37	16. 7

Atlantic.	Pacific.	Indian.	Longitude.	H. W. Gr. T.
			H. M.	H. M.
Belleisle			0.12 W.	16.12 Lal.
		Palmiras Pt.	18.12	16.12
		Port Cornwallis, And.	17.49	16.19
Rochelle			0. 5	16.20
Vannes			0.11	16.24
St. Paul de Leon / Morlaix			0.16	16.38 or 18. 0
Rochefort			0. 4	16.49
Bear Island			5.20	17.20
		Christmas Island	19.24	17.24
	Chiloe		5. 0	17.30
Cape Clear			0.38	17.38
	Annamocka		11.39	17.39
	St. Peter and Paul / Awatsha		13.25	18. 1
Kinsale			0.34	18. 4
Eddystone			0.18	18.18
Falmouth			0.20	18.20
	Rotterdam I.		12.21	18.21
Drake's Island, Plymouth			0.17	18.32
Plymouth			0.17	18.47
Avranches			0. 5	18.35
	Eaoowe		11.38	18.38
St. Maloes			0. 8	18.38 (50 ft.)
Londonderry			0.29	18.59
	Tongatabu		12.20	19.10
Granville			0. 6	19.21
	Pudyona		13. 1	19.31
	St. Francisco		8. 8	19.33
Cork			0.34	19.34
Bristol.			0.10	7.10
Barfleur			0. 5	20. 5
Cherbourg			0. 6	20. 6
	Venus Pt., Otah.		9.58	20.36
		Mauritius	20.10	20.40
Lizard			0.21	20.45
	Nootka Sound		8.27	20.47
Guernsey			0. 9	20.54
		Pulo Condore	18.53	21. 9
		Calcutta	18. 6	21.11
		Seychelles, Alm.	20.18	21.12
Stromness			0.14	21.14
	New Zealand Q. C. S.		12.13	21.23
	New Zealand Dusky B.		12.55	23.54
Honfleur			0. 1 E.	21.29
Havre			0. 0	21.30
		Socotora and C. Guardafui.	20.30 W.	21.30
Caen			0. 1	21.31
	Ulietea.		10. 6	21.39 to 21.42
	Huaheine		10. 4	21.55
Shoreham			0. 1	22. 1
		Foul Pt., Mad.	20.41	22. 1
	Botany Bay		13.55	21.55 or 23.55
St. Valery en Caux			0. 3 E.	22.12

Atlantic.	Pacific.	Indian.	Longitude.	H. W. Gr. T.
			H. M.	H. M.
		Macao	16.26 W.	22.16
St. Valery sur Somme			0. 6 E.	22.24
Dunnose			0. 5 W.	22.20
Brighton			0. 1	22.31
Dublin			0.25 W.	22.35 ?
Abbeville			0. 7 E.	22.57
Beachy Head			0. 1 E.	22.59
Cowes			0. 6 W.	23. 5
Needles			0. 6 W.	23. 6
Anholt			0.47 E.	23.13
Boulogne			0. 6	23.24
Hastings			0. 3	23.27
Deal Castle			0. 6	23.39
Dover			0. 5	23.40
Dungeness			0. 4	23.41
Dieppe			0. 4	23.41
		Almirantes { Eagle I.	21.28 W.	23.48
		Almirantes { Curreuse	21.17	25.27
Portsmouth			0. 4 W.	23.49
Ostend			0.12 E.	24. 3
Nieuport			0.11	24. 4
Gravelines			0. 8 E.	24 22
Aberdeen			0. 9 W.	24.54
Alderney			0. 9	25. 0 ?
Bergen			0.21 E.	25. 9
		False Bay	22.45 W.	25.16
Drontheim			0.41 E.	25.34
Rouen			0. 4 W.	25.41
Aberdeen			0. 9	25.54
North Cape			1.43 E.	26. 1
Leith and Edinburgh			0.13 W.	26.33
Amsterdam			0.19 E.	27.11
Rotterdam			0.18	27.12
London Bridge			0. 0	27.15
Archangel			2.36 E.	27.24
Bordeaux			0. 2 W.	27.32
Hamburg			0.40 E.	29.20
Bremen			0.35 E.	29.25
Antwerp			0.18 E.	30.12
Scolt Head			0. 3	30.17
Lynn			0. 2	30.43
Hague			0.17	31.58
Lowestoft			0. 7	34.23
London Bridge			0. 0	(39.45)

It may be immediately inferred from this table, first, That the line of contemporary tides is seldom in the exact direction of the meridian, as it is supposed to be universally in the theory of Newton and of Laplace; except, perhaps, the line for the 21st hour in the Indian Ocean, which appears to extend from Socotora to the Almirantes and the Isle of Bourbon, lying

nearly in the same longitude. Secondly, That the southern extremity of the line advances as it passes the Cape of Good Hope, so that it turns up towards the Atlantic, which it enters obliquely, so as to arrive, nearly at the same moment, at the Island of Ascension, and at the Island of Martin Vaz, or of the Trinity. Thirdly, After several irregularities about the Cape Verd Islands, and in the West Indies, the line appears to run nearly east and west from St. Domingo to Cape Blanco, the tides proceeding due northwards; and then, turning still more to the right, the line seems to run N. W. and S. E., till at last the tide runs almost due east up the British Channel, and round the North of Scotland into the Northern Ocean, sending off a branch down the North Sea to meet the succeeding tide at the mouth of the Thames. Fourthly, Towards Cape Horn again there is a good deal of irregularity; the hour lines are much compressed between South Georgia and Tierra del Fuego, perhaps on account of the shallower water about the Falkland Islands and South Shetland.

In the fifth place, At the entrance of the Pacific Ocean, the tides seem to advance very rapidly to New Zealand and Easter Island; but here it appears to be uncertain whether the line of contemporary tide should be drawn nearly N. and S. from the Gallapagos to Tierra del Fuego, or N. E. and S. W. from Easter Island to New Zealand; or whether both these partial directions are correct: but on each side of this line there are great irregularities, and many more observations are wanting before the progress of the tide can be traced with any tolerable accuracy among the multitudinous islands of the Pacific Ocean, where it might have been hoped that the phenomena would have been observed in their greatest simplicity, and in their most genuine form.

Lastly, Of the Indian Ocean, the northern parts exhibit great irregularities, and among the rest they afford the singular phenomenon observed by Halley in the Port of Tonkin, and explained by Newton in the 'Principia:' the southern parts are only remarkable for having the hour lines of contemporary tides considerably crowded between New Holland and the Cape of Good Hope, as if the seas of these parts were shallower than elsewhere.

These inferences respecting the progress of the tides are not advanced as the result of any particular theory, nor even as the only ones that might possibly be deduced from the table: thus the supposition that the direction in which the tides advance must be perpendicular to the hour lines of contemporary tides, is not by any means absolutely without exception, since a quadrangular lake, with steep shores in the direction of the meridian, would have the times of high water the same for every point of its eastern or western halves respectively, and there could be no correctly defined direction of the hour lines in such a case. But if any portion of the sea could be considered as constituting such a lake, its properties would be detected by a sufficient number of observations of high water, and the existing table does not appear to indicate any such cases that require to be otherwise distinguished than as partial irregularities. There may also be some doubt respecting the propriety of the addition of 12½ hours that has been made to the time of high water in the north-eastern parts of the Atlantic: but it seems extremely improbable that the same tide should travel north-easterly into the English Channel and into the Northern Ocean, and at the same time westerly across the Atlantic, as it must be supposed to do, if it were considered as primarily originating in the neighbourhood of the Bay of Biscay; on the other hand, the bending of the great wave round the continents of Africa and Europe seems to be very like the sort of refraction which takes place on every shelving coast with respect to the common waves, which, whatever may have been their primitive origin, acquire always, as they spread, a direction more and more nearly parallel to that of the coast which they are approaching: and the suppositions which have been here advanced respecting the succession of the tides in different ports, allowing for the effect of a multitude of irregularities proceeding from partial causes, appear to be by far the most probable that can be immediately inferred from the table, at least in its present state of imperfection.

SECT. II.—*Of the Disturbing Forces that occasion the Tides.*

Since the phenomena of the tides, with regard to their progress through the different oceans and seas, as they exist in the actual state of the earth's surface, appear to be too complicated to allow us to hope to reduce them to computation by means of any general theory, we must, in the next place, confine our attention to the order in which the successive changes occur in any single port, and having determined the exact magnitude of the forces that tend to change the form of the surface of the ocean at different periods, and having also examined the nature of the vibratory motions, of which the sea, or any given portion of it, would be susceptible, in the simplest cases, after the cessation of the disturbing forces, we must afterwards endeavour to combine these causes so as to adapt the result to the successive phenomena, which are observed at different times in any one port.

THEOREM A.* ('E.'—Nich. Journ., Jul. 1813.) The disturbing force of a distant attractive body, urging a particle of a fluid in the direction of the surface of a sphere, varies as the sine of twice the altitude of the body.

The mean attraction, exerted by the sun and moon on all the separate particles composing the earth, is exactly compensated by the centrifugal force derived from the earth's annual revolution round the sun, and from its monthly revolution round the common centre of gravity of the earth and moon: but the difference of the attractions, exerted at different points of the earth, must necessarily produce a disturbing force, depending on the angular position of the point with regard to the sun or moon, since the centrifugal force is the same for them all; the disturbing force being constantly variable for any one point, and depending partly on the difference of the distance of the point from the mean distance, and partly on the difference of the direction of the luminary, from its direction with respect to the centre, or, in other words, on its parallax.

It will be most convenient for computation to consider both

* Supra, p. 121 and p. 278.

these forces, for a sphere covered with a fluid, as referred to the direction of the circumference of the sphere, which will differ but little from that of the fluid; and it will appear that both of them, when reduced to this direction, will vary as the product of the sine and cosine of the distance from the diameter pointing to the luminary, that is as half the sine of twice the altitude: for the difference of gravitation, which depends on the difference of the distance, will always vary as the sine of the distance from the bisecting plane perpendicular to that diameter, and will be reduced to the direction of the surface, by diminishing it in the ratio of the cosine to the radius: and the effect of the difference of direction will be originally proportional to the sine of the distance from the diameter, and will in like manner be expressed, when reduced, by the product of the sine and cosine; and each force, thus reduced, will be equal, where it is greatest, to half of its primitive magnitude, since $\sin. 45° \cos. 45° = \frac{1}{2}$. "Thus, the gravitation towards the moon at the earth's surface is to the gravitation towards the earth as 1 to 70 times the square of $60\frac{1}{2}$, or to 256,217: and the former disturbing force is to the whole of this as 2 to $60\frac{1}{2}$ at the point nearest the moon, and the second as 1 to $60\frac{1}{2}$ at the equatorial plane, and the sum of both, reduced to the direction of the circumference, where greatest, as 3 to 121; that is, to the whole force of the earth's gravitation as 1 to 10,334,000; and in a similar manner we find, that the whole disturbing force of the sun is to the weight of the particles as 1 to 25,736,000." Or, if we call the moon's horizontal parallax p, and substitute $\frac{1}{p}$ for the distance, the whole of the lunar disturbing force in the direction of the surface will be $\frac{3}{2} \cdot \frac{p^3}{70} = \frac{3}{140} p^3$; or, if z be the moon's zenith distance from any point of the surface, $f = \frac{3}{70} p^3 \sin. z \cos. z$.

THEOREM B.* [F.] The inclination of the surface of an oblong spheroid, slightly elliptical, to that of the inscribed sphere, varies as the sine of twice the distance from the circle of contact; and a particle resting on any part of it, without

* Supra, p. 121 and p. 278.

friction, may be held in equilibrium by the attraction of a distant body [situated in the direction of the axis].

If a sphere be inscribed in an oblong spheroid, the elevation of the spheroid above the sphere must obviously be proportional, when measured in a direction parallel to the axis of the spheroid, to the ordinate of the sphere, that is, to the sine of the distance from its equator; and when reduced to a direction perpendicular to the surface of the sphere, it must be proportional to the square of that sine; and the tangent of the inclination to the surface of the sphere, which is equal to the fluxion of the elevation divided by that of the circumference, must be expressed by twice the continual product of the sine, the cosine, and the ellipticity, or rather the greater elevation, e, the radius being considered as unity: so that the elevation e will also express the tangent of the inclination where it is greatest, since $2 \sin. 45° \cos. 45° = 1$; and the inclination will be everywhere as the product of the sine and cosine.

If, therefore, the density of the elevated parts be considered as evanescent, and their attraction be neglected, there will be an equilibrium, when the ellipticity is to the radius as the disturbing force to the whole force of gravitation: for each particle situated on the surface, will be actuated by a disturbing force tending towards the pole of the spheroid, precisely equal and contrary to that portion of the force of gravitation which urges it in the opposite direction down the inclined surface. Hence, if the density of the sea were supposed inconsiderable in comparison with that of the earth, the radius being 20,839,000 feet, the greatest height of a lunar tide in equilibrium would be 2·0166 feet, and that of a solar tide ·8097: that is, supposing the moon's horizontal parallax about 57′, and her mass $\frac{1}{70}$ of that of the earth.

THEOREM C.* [G.] The disturbing attraction of the thin shell, contained between a spheroidical surface and its inscribed sphere, varies in the same proportion as the inclination of the surface, and is to the relative force of gravity depending on that inclination, as three times the density of the shell to five times that of the sphere.

* Supra, p. 124 and p. 279.

We may imagine the surface of the sphere to be divided by an infinite number of parallel and equidistant circles, beginning from any point at which a gravitating particle is situated, and we may suppose all these circles to be divided by a plane perpendicular to the meridian of the point, and consequently bisecting the equatorial plane of the spheroid: it is obvious, that, if the elevations on the opposite sides of the plane be equal at the corresponding points of each circle, no lateral force will be produced: but when they are unequal, the excess of the elevated matter on one side above that of the other side will produce a disturbing or lateral force. Now, the elevation being everywhere as the square of the distance x from the equatorial plane, we may call it ex^2, and the difference, corresponding to any point of that semicircle which is the nearer to the pole of the spheroid, will be $e(x'^2-x''^2)=e(x'+x'')(x'-x'')$. But $x'+x''$ is always twice the distance of the centre of the supposed circle from the equatorial plane; and the distance of this centre from that of the sphere will be cos. ψ, if ψ be the angular distance of the circle from its pole; and calling φ the distance of this pole from the equatorial plane of the spheroid, the distance in question will be cos. ψ sin. φ, and $x'+x''=2$ cos. ψ sin. φ: and the difference $x'-x''$ is twice the actual sine of the arc θ in the supposed circle, that is, twice the natural sine, reduced in the ratio of unit to the radius of this circle, which is sin. ψ, reduced again to a direction perpendicular to the equatorial plane; whence $x'-x''=2$ sin. θ sin. ψ cos. φ: and $x'^2-x''^2=4$ sin. θ sin. ψ cos. ψ sin. φ cos. φ. Hence it follows, that, in different positions of the gravitating particle, the effective elevation at each point of the surface, similarly situated with respect to it, is as the product of the sine and cosine of its angular distance φ from the equatorial plane, the other quantities concerned remaining the same in all positions. But the inclination of the surface of the spheroid, as well as the original disturbing force, varies in the same proportion of the product of the sine and cosine of the distance φ: consequently, the sum of this disturbing attraction and the original force will also vary as the inclination of the surface, and may be in equilibrium with the tendency to descend towards the centre, pro-

vided that the ellipticity be duly commensurate to the density of the elevated parts.

Now, in order to find the actual magnitude of the disturbing attraction for a shell of given density, we must compute the fluent of $4e \sin.\theta d\theta \sin.\psi \cos.\psi d\psi \sin.\varphi \cos.\varphi$, reduced first according to the distance and direction of each particle from the given gravitating particle; and we must compare the fluent with $\frac{4}{3}\pi$, the attraction of the whole sphere at the distance of the radius or unity. But for the angle θ, the portion of the force acting in the common direction of $\sin.\theta$ is to the whole attraction at the same distance as $\sin.\theta$ to 1, so that the attractive force of any point of the semicircle will be $4e \sin.^2\theta \sin.\psi \cos.\psi \sin.\varphi \cos.\varphi$, and its fluxion will be as $\sin.^2\theta d\theta$, of which the fluent is $\frac{1}{2}\theta - \frac{1}{2}\sin.\theta \cos.\theta$, or when $\theta = 180°$, $\frac{1}{2}\pi$, and $\frac{1}{2}\pi \sin.\psi$ will express the effect of the disturbing attraction of the semicircles, of which $\sin.\psi$ is the radius, reduced to the direction of the middle point, of which the distance is $2 \sin.\frac{1}{2}\psi$; the reduction for this distance is as its square to 1; and for the direction, as the distance to $\sin.\psi$, together making the ratio of $\frac{\sin.\psi}{8 \sin.^3\frac{1}{2}\psi}$, and the ultimate fluxion of the force will be $2e\pi \sin.\psi \sin.\varphi \cos.\varphi \sin.\psi \cos.\psi \frac{\sin.\psi}{8 \sin.^3\frac{1}{2}\psi} \cdot d\psi = 2e\pi \frac{\sin.^3\psi \cos.\psi}{8 \sin.^3\frac{1}{2}\psi} \sin.\varphi \cos.\varphi d\psi$; but $\sin.\psi = 2 \sin.\frac{1}{2}\psi \cos.\frac{1}{2}\psi$, and the fraction becomes $\frac{8 \sin.\frac{1}{2}\psi\, ^3\cos.^3\frac{1}{2}\psi}{8 \sin.^3\frac{1}{2}\psi} \cos.\psi = \cos.^3\frac{1}{2}\psi \cos.\psi = \cos.^3\frac{1}{2}\psi (\cos.^2\frac{1}{2}\psi - \sin.^2\frac{1}{2}\psi) = \cos.^5\frac{1}{2}\psi - \cos.^3\frac{1}{2}\psi + \cos.^5\frac{1}{2}\psi = 2\cos.^5\frac{1}{2}\psi - \cos.^3\frac{1}{2}\psi$. Now, taking the fluent from $\psi = 0$ to $\psi = 180°$, we have $2 \int' \cos.^5\frac{1}{2}\psi\, 2d\frac{1}{2}\psi = \frac{8}{5} \cdot \frac{4}{3}$, and $\int' \cos.^3\frac{1}{2}\psi \times 2d\frac{1}{2}\psi = \frac{4}{3}$: the difference being $\frac{3}{5} \cdot \frac{4}{3} = \frac{4}{5}$; whence the fluent of the force is found $2e\pi \sin.\varphi \cos.\varphi \times \frac{4}{5} \times \frac{1}{n}$, calling the density of the fluid $\frac{1}{n}$; or, where it is greatest, $\sin.\varphi \cos.\varphi$ being $= \frac{1}{2}$, $\frac{4}{5}\frac{e\pi}{n}$, while the attraction of the sphere itself is $\frac{4}{3}\pi$, which is to $\frac{4}{5}\frac{e\pi}{n}$ as 1 to $\frac{3e}{5n}$; and since the elevation

e expresses also the maximum of the relative force of gravity depending on the tangent of the inclination (Theorem B), it is obvious that the disturbing attraction $\frac{3e}{5n}$ must be to the relative force e as $\frac{3}{n}$ to 5.

Corollary 1. If $n = 1$, as in a homogeneous fluid sphere or spheroid, the disturbing attraction becomes $\frac{3}{5}e$, and this attraction, together with the primitive force f, must express the actual elevation e, or $\frac{3}{5}e + f = e$, whence $f = \frac{2}{5}e$, and $e = \frac{5}{2}f$, giving 2·024 and 5·042 for the magnitude of the solar and lunar tides, when $f =$ ·8097 and 2·0166 respectively. But this is obviously far from the actual state of the problem.

Corollary 2. Supposing $n = 5·4$ (See *Journ. R. I.*, Apr. 1820(,* we have $\frac{3e}{27} + f = e$, and $e = \frac{27}{24}f = \frac{9}{8}f$; so that the height of the primitive tides of an ocean of water, covering the whole surface of the earth, such as it actually is, ought to be ·911 for the solar, and 2·27 for the lunar disturbing force: that is, supposing the sea to be without inertia, so as to accommodate itself at once to the form of equilibrium. But, in the actual state of the irregularities of the seas and continents, it is impossible to pay any regard to this secondary force, since the phenomena do not justify us in supposing the general form of the surface of the ocean such as to give rise to it.

Theorem D.† [H.] When the horizontal surface of a liquid is elevated or depressed a little at a given point, the effect will be propagated in the manner of a wave, with a velocity equal to that of a heavy body which has fallen through a space equal to half the depth of the fluid, the form of the wave remaining similar to that of the original elevation or depression. *Illustr. Celest. Mech.* 378, p. 318.

Scholium. The demonstration of this theorem implies that water is incompressible, and that the pressure of each particle placed on the surface is instantaneously communicated through the whole depth of the fluid to the bottom. These suppositions

* Supra, p. 81.

† Supra, p. 279.

are not indeed strictly accurate in any case, but they introduce no sensible error when the surface of the wave similarly affected is large in comparison with the depth of the fluid. A modern author,* of celebrity, seems to have taken it for granted, that the pressure is propagated with the same velocity downwards and laterally; at least, if such is not his meaning, he has been somewhat unfortunate in the choice of his expressions; but there seems no reason whatever why water should communicate force more slowly when it is perfectly confined than ice would do; and the divergence of the pressure of a certain portion of the surface of water, elevated a little, for example, above the rest, may be compared to the *divergence* of a sound entering into a detached chamber by an aperture of the same size with the given surface, which is probably *small* in comparison with its direct motion, but *equally rapid*, and in both cases depending on the modulus of the elasticity of the medium.

THEOREM E. [I.] † A wave of a symmetrical form, with a depression equal and similar to its elevation, striking against a solid vertical obstacle, will be reflected, so as to cause a part of the surface, at the distance of one fourth of its whole breadth, to remain at rest; and if there be another opposite obstacle at twice that distance, there may be a perpetual vibration between the surfaces, the middle point having no vertical motion. *Nat. Phil.* I. p. 289, 777.

Scholium 1. The elevation and depression of a spheroid, compared with the surface of the sphere of equal magnitude, exhibit a symmetrical wave in the sense of the proposition: and it is not necessary that the shore should be very rocky or perpendicular, in order to produce a strong reflection; for even the vibration of the water in the bottom of a common hemispherical basin is considerably permanent.

Corollary 2. The vibrations of the water supposed to be contained in a canal, following the direction of the equator, and 90° in length, would be synchronous with the passage of a wave 180° in breadth, over any point of a canal of the same depth, and surrounding the whole globe.

Scholium 2. It has been usual to consider the elevation of the tides, as identical with that of an oblong spheroid, mea-

* Supra, p. 142.
† Supra, p. 279.

sured at its vertex, and therefore as amounting to twice as much as the depression of the same spheroid at the equator, considered in relation to the mean height belonging to a sphere of the same magnitude: but the supposition is by no means applicable to the case of a globe covered partially and irregularly with water, so that in almost all cases of actual tides the elevation must be considered as little if at all greater than the depression, as far as this cause only is concerned; there are, however, some other reasons to expect that the elevation of the great wave might often arrive at a distant port in somewhat greater force than the depression.

Theorem F.* [K.] The oscillations of the sea and of lakes, constituting the tides, are subject to laws exactly similar to those of pendulums capable of performing vibrations in the same time, and suspended from points which are subjected to compound regular vibrations, of which the constituent periods are completed in half a lunar and half a solar day [or in some particular cases a whole day].

Supposing the surface of the sea to remain at rest, each point of it would become alternately elevated and depressed, in comparison with the situation in which it might remain in equilibrium; its distance from this situation varying according to the regular law of the pendulum (see Theorem B); and like all minute vibrations, it will be actuated by forces indirectly dependent on, and proportional to, this distance; so that it may be compared to a pendulous body remaining at rest in the vertical line, about which its point of suspension vibrates, and will consequently follow the motion of the temporary horizon, in the same manner as the pendulum follows the vibration of its point of suspension, either with a direct or a retrograde motion, according to circumstances which will be hereafter explained: the operation of the forces concerned being perfectly analogous, whether we consider the simple hydrostatic pressure depending on the elevation, or the horizontal pressure, derived from the inclination of the surface, or the differential force immediately producing elevation and depression, depending on the variation of the horizontal pressure, and proportional to the curvature of the surface. It becomes therefore neces-

* Supra, p. 280.

sary for the theory of the tides, to investigate minutely the laws of these compound and compulsory vibrations; which, together with the resistances affecting them, will be the subject of the next section.

SECT. III.—*Of the Effects of Resistance in Vibrating Motions, whether Simple or Compound.*

THEOREM G. If $\mathrm{d}w + A\mathrm{d}s + Bs\mathrm{d}s + Dw\mathrm{d}s = 0$, we have $e^{Ds}\left(w + \frac{B}{D}s + \frac{AD-B}{DD}\right) = c$; hl$e$ being $= 1$.

Scholium. For the better understanding of the mode of investigation which will be employed in these propositions, it will be proper to premise some remarks on the investigation of fluxional equations by means of multipliers. A person, unacquainted with the language of modern mathematicians, would naturally understand, by a "criterion of integrability," some mode of distinguishing an expression that would be integrated, from one that was untractable: while, in fact, this celebrated criterion relates only to the accidental form in which the expression occurs, and not to its essential nature. If we take, for instance, the well-known case of the fluxion of hl $\frac{x}{y} = \text{hl}x - \text{hl}y$, we have $\frac{\mathrm{d}x}{x} - \frac{\mathrm{d}y}{y} = \frac{y\mathrm{d}x - x\mathrm{d}y}{xy}$; and making this $=0$, we have also $y\mathrm{d}x - x\mathrm{d}y = 0$; and this expression no longer fulfils the conditions of integrability, until we multiply it again by $\frac{1}{xy}$, and restore it to its perfect form. The direct investigation of such a multiplier is generally attended by insuperable difficulties; and the best expedient, in practical cases, is to examine the results of the employment of such multipliers, as are most likely to be concerned in the problem, with indeterminate coefficients, and to compare them with the equations proposed. In common cases, the finding of fluents, when only one variable quantity is concerned, requires little more than the employment of a Table such as that of Meier Hirsch, or that which constitutes the Article FLUENTS in this *Supplement*;* and the demonstration of the truth of the solution is in general furnished at

* This article, which is little more than an abridgment of the Integraltafeln of Hirsch, has not been reprinted.—*Note by the Editor.*

once, for each case, by taking the fluxion of the quantity inserted in the Table as the fluent: but for the separation of different variable quantities, where they are involved with each other, the employment of proper multipliers is one of the most effectual expedients; and it is still more essential to the solution of equations between fluxions of different orders, or their coefficients. Such equations require in general to be compared with some multiple of the exponential quantity e^{mt}, which affords fluxions of successive orders, that have simple relations to each other, especially when dt is considered as constant. The multiples of sin. Ct, and cos. Ct, are always very useful in such investigations, and for a similar reason; but the solutions that they afford are commonly less comprehensive than the former; though they are often simpler, and more easily obtained. It is not, however, necessary that the exponent of the multiplier should flow uniformly, as will appear from the first example of a problem which has been solved by Euler in his *Mechanics:* the subsequent examples will possess somewhat more of novelty.

Demonstration. The fluxion of $e^{ns}(w+ps+q)$ is $e^{ns}(\mathrm{d}w+p\mathrm{d}s+(nw+nps+nq)\,\mathrm{d}s) = e^{ns}(\mathrm{d}w+(p+nq)\mathrm{d}s+nps\mathrm{d}s+nw\mathrm{d}s)$; and comparing with this $e^{ns}(\mathrm{d}w+A\mathrm{d}s+Bs\mathrm{d}s+Dw\mathrm{d}s)$, we have $n=D$, $np=B$, and $p=\frac{B}{n}=\frac{B}{D}$; and, lastly, $p+nq=A$, $q=\frac{A-p}{n}=\frac{AD-B}{DD}$; consequently the fluxion of $e^{Ds}\left(w+\frac{B}{D}s+\frac{AD-B}{DD}\right)$ is equal to nothing, and that quantity is constant, or equal to c.

Example. Let the given equation be that of a cycloidal pendulum, moving with a resistance proportional to the square of the velocity, or $\frac{\mathrm{dd}s}{\mathrm{d}t^2}+Bs-D\frac{\mathrm{d}s^2}{\mathrm{d}t^2}=0$.

Scholium 2. The space s being supposed to begin at the lowest point of the curve, the fluxion ds is negative during the descent on the positive side, and the force dd$s$ is consequently negative, and equal, when there is no resistance, to Bs, B being a positive coefficient, equivalent, in the case of gravitation, to $\frac{2g}{l}$ or $\frac{32}{l}$, l being the length of the pendulum, and g

the descent of a falling body in the first second. The coefficient $-D$ is negative, because the resistance acts in a contrary direction to that of the force Bs, as long as s remains positive, and coincides with it on the negative side. But in the return of the pendulum the signs are changed; so that the equation can only be applied to a single vibration: since the two forces in question oppose each other in the same points of the curve in which they before agreed, while the square $\frac{ds^2}{dt^2}$ must always remain positive.

Solution. If we multiply the given equation by ds, and make the square of the velocity, or $vv = w = \frac{ds^2}{dt^2}$, we have $ds\frac{dds}{dt^2} + Bsds - D\frac{ds^2}{dt^2}ds = 0 = \frac{1}{2}dw + Bsds - Dwds$, and $dw + 2Bsds - 2Dwds = 0$; which, compared with the theorem, gives us 0 for A, $2B$ for B, and $-2D$ for D; and the solution becomes $e^{-2Ds}\left(w - \frac{B}{D}s - \frac{B}{2DD}\right) = c$, or $w = \frac{B}{D}s + \frac{B}{2DD} + ce^{2Ds}$; and if $w = 0$ when $s = \lambda$, we have $\frac{B}{D}\lambda + \frac{B}{2DD} + ce^{2D\lambda} = 0$, or, putting $\frac{B}{D}\lambda + \frac{B}{2DD} = \beta$, $\beta + ce^{2D\lambda} = 0$, and $c = -\beta e^{-2D\lambda}$; β being also $= \frac{B\gamma}{2DD}$, if $\gamma = 1 + 2D\lambda$. We may also substitute σ for $\lambda - s$, and ce^{2Ds}, $= -\beta e^{2D(s-\lambda)}$, will become $= -\beta e^{-2D\sigma}$, and $w = \frac{B}{D}s + \frac{B}{2DD} - \beta e^{2D\sigma} = \frac{B}{2DD}(1 - \gamma e^{-2D\sigma} + 2Ds)$. Now $e^{-2D\sigma} = 1 - 2D\sigma + 2D^2\sigma^2 - \frac{4}{3}D^3\sigma^3 + \frac{2}{3}D^4\sigma^4 - \ldots$; and $(1 + 2D\lambda)e^{-2D\sigma} = 1 + 2D\lambda - 2D\sigma - 4D^2\lambda\sigma + 2D^2\sigma^2 + 4D^3\lambda\sigma^2 - D\tfrac{4}{3}{}^3\sigma^3 - \ldots$; whence $w = \frac{B}{2DD}(2Ds - 2D(\lambda - \sigma) + 4D^2\lambda\sigma - 2D^2\sigma \ldots) = \frac{B}{2DD}(4D^2\lambda\sigma - 2D^2\sigma^2 - 4D^3\lambda\sigma^2 \ldots) =$
$B(2\lambda\sigma - \gamma\sigma^2 + \frac{2}{3}D\gamma\sigma^3 - \frac{1}{3}D^2\gamma\sigma^4 + \ldots)$.

Corollary 1. From this solution we obtain the point at which the velocity is greatest; and, by reversing the equation, we may also find the extent of the vibration. For when $dw = 0$, we have $Bsds = Dwds$, and $Bs = Dw$, which is the obvious expression of the equality of the resistance to the propelling force. Putting

the greatest value of $w=\varkappa$, and the corresponding value of $s=\varsigma$, we have $\varkappa=\frac{B}{D}\varsigma+\frac{B}{2DD}+ce^{2D\varsigma}=\frac{B}{D}\varsigma$, since $B\varsigma=D\varkappa$, and $\frac{B}{2DD}=-ce^{2Ds}=\frac{B\gamma}{2DD}e^{2D(\varsigma-\lambda)}$; whence $\frac{1}{\gamma}=e^{2D(\varsigma-\lambda)}$, and $\gamma=e^{2D(\lambda-\varsigma)}$; consequently $\mathrm{hl}\,\gamma=2D(\lambda-\varsigma)=\mathrm{hl}\,(1+2D\lambda)$, and $2D\varsigma=2D\lambda-\mathrm{hl}\,(1+2D\lambda)$, and $\varsigma=\frac{1}{2D}\left(2D^2\lambda^2-\frac{8}{3}D^3\lambda^3+\ldots\right)=D\lambda^2-\frac{4}{3}D^2\lambda^3+\ldots$ And since $\varkappa=\frac{B}{D}\varsigma$, we have $\varkappa=\frac{B}{2DD}(2D\lambda-\mathrm{hl}\,[1+2D\lambda])$.

Lemma. For the reversion of a series, or of a finite equation, if $z=ax+bx^2+cx^3+\ldots$, we have $x=\frac{1}{a}z-\frac{b}{a^3}z^2+\frac{2b^2-ac}{a^5}z^3-\frac{5b^3-5abc+a^2d}{a^7}z^4+\frac{14b^4-21ab^2c+6a^2bd+3a^2c^2-a^3e}{a^9}z^5-\ldots$

The proof of this well-known formula is the most readily obtained by means of a series with indeterminate coefficients, such as $x=Az+Bz^2+\ldots$, which, by actual involution, and by comparison with the proposed series, will give the required values of the coefficients, as expressed in this Lemma.

Corollary 2. When $w=o$, we obtain from its value, divided by $B\sigma$, the equation $2\lambda=\gamma\sigma-\frac{2}{3}D\gamma\sigma^2+\frac{1}{3}D^2\gamma\sigma^3-\ldots$; and by reversing this series, we have $\sigma=\frac{2\lambda}{\gamma}+\frac{8}{3\gamma^2}D\lambda^2+\frac{40}{9\gamma^3}D^2\lambda^3\ldots$, or $\sigma=2\lambda-\frac{4}{3}D\lambda^2+\ldots$; the difference of the arcs of descent and ascent being $\frac{4}{3}D\lambda^2$, and the difference of two successive vibrations $\frac{8}{3}D\lambda^2$, when the resistance is very small; this difference being also $\frac{8}{3}\varsigma$; so that the displacement of the point of greatest velocity is $\frac{3}{8}$ of the difference of the successive vibrations.

Scholium 2. If K be the value of w when Dw would be equal to the force of gravity, and $DK=Bl=2g$, we have $D=\frac{2g}{K}$, or H being the height from which a body must fall to acquire the velocity $\surd K$, since $K=4gH$, $D=\frac{1}{2H}$, and $2D=\frac{1}{H}$.

Scholium 3. It is natural to imagine that we might obtain the time from the equation expressing the velocity in terms of the space, if we merely expanded the value of $\frac{1}{\sqrt{w}}$ into a new series, by means of the Newtonian theorem: but the fluents thus obtained for the expression of the time are deficient in convergency; and a similar difficulty would occur if we expressed σ in terms of w by reversing the series, and divided its fluxion by $\sqrt{w}$. The ingenuity of Euler has, however, devised a method of avoiding these inconveniences, by supposing the time to begin at the point where the velocity is a maximum; and it will be necessary, in this investigation, to follow his steps, with some slight variations.

Corollary 3. In order to find the time of vibration, we take $s - \varsigma = \tau$, and $\varkappa - w = z$, then $s = \tau + \varsigma$, $\sigma = \lambda - \varsigma - \tau$, $w = \frac{B}{2DD}(1 - \gamma e^{-2D(\lambda-\varsigma-\tau)} + 2D[\varsigma+\tau])$, and $\varkappa$ being $= \frac{D}{B}\varsigma$, $z = -\frac{B}{2DD} - \frac{B}{D}\tau + \frac{B\gamma}{2DD}e^{2D(\varsigma-\lambda)}e^{2D\tau}$; but we have seen that $e^{2D}(\varsigma-\lambda) = \frac{1}{\gamma}$, and $-z$ becomes $\frac{B}{2DD} + \frac{B}{D}\tau - \frac{B}{2DD}e^{2D\tau}$ $= \frac{B}{D}\tau - \frac{B}{2DD}\left(2D\tau + 2D^2\tau^2 + \frac{4}{3}D^3\tau^3 + \frac{8}{12}D^4\tau^4 + \ldots\right) =$ $\frac{B}{D}\tau - \frac{B}{D}\tau - B\tau^2 - \frac{2}{3}BD\tau^3 - \ldots = -B\tau^2 - \frac{2}{3}BD\tau^3 - \frac{1}{3}BD^2\tau^4 - \ldots$, and $\frac{z}{B} = \tau^2 + \frac{2}{3}D\tau^3 + \frac{1}{3}D^2\tau^4 + \ldots$. In order to reverse this series, we must put $\frac{z}{B} = y^2$, and $\tau = \text{A}y + \text{B}y^3 + \text{C}y^3 + \ldots$, and by substituting the powers of this series for those of τ in the value of $\frac{z}{B} = y^2 = \tau^2 + \text{P}\tau^3 + \text{Q}\tau^4 + \ldots$, we find $\text{A} = 1$, $\text{B} = -\frac{1}{2}\text{P}$, $\text{C} = \frac{5}{8}\text{P}^2 - \frac{1}{2}\text{Q}\ldots$; and $\tau = y - \frac{1}{3}Dy^2 + \frac{1}{9}D^2y^3 - \ldots$. Hence $\mathrm{d}\tau = \mathrm{d}y - \frac{2}{3}Dy\mathrm{d}y + \frac{1}{3}D^2y^2\mathrm{d}y - \ldots$; and this fluxion, divided by the velocity $v = \sqrt{(\varkappa - z)}$, will be the fluxion of the time; or, since $\mathrm{d}\frac{z}{B} = 2y\mathrm{d}y$ and $\mathrm{d}y = \frac{\mathrm{d}z}{2By} = \frac{\mathrm{d}z}{2\sqrt{B}\sqrt{z}}$, $\mathrm{d}t = \sqrt{\frac{1}{B}} \cdot \frac{\mathrm{d}z}{2\sqrt{(\varkappa z - zz)}} - \frac{D}{3B}\frac{\mathrm{d}z}{\sqrt{(\varkappa - z)}} + \frac{D^2}{6B\sqrt{B}} \cdot \frac{z\mathrm{d}z}{\sqrt{(\varkappa z - zz)}} - \ldots$: and the fluent becomes $t = \frac{1}{2\sqrt{B}}$ arc v sin. $\frac{2z}{\varkappa} - \frac{2D}{3B}\sqrt{(\varkappa - z)} +$

$\frac{D^2}{6B\sqrt{B}}\left(\frac{1}{2}\varkappa \text{ arc } v \text{ sin. } \frac{2z}{\varkappa} - \sqrt{[\varkappa z - z^2]}\right) \ldots$; the value of which, taken from $z = 0$ to $z = \varkappa$, is $\frac{\pi}{2\sqrt{B}} - \frac{2D}{3B}\sqrt{\varkappa} + \frac{D^2}{6B\sqrt{B}}(\frac{1}{2}\varkappa\pi) \ldots$. If we now make τ negative, for the ascent of the pendulum, the coefficients P, R, ..., B, D, ..., will change their signs, and the value of t will be $\frac{\pi}{2\sqrt{B}} + \frac{2D}{3B}\sqrt{\varkappa} + \frac{D^2}{12B\sqrt{B}}\varkappa\pi + \ldots$, the sum of both being $\frac{\pi}{\sqrt{B}} + \frac{D^2\varkappa\pi}{6B\sqrt{B}} + \ldots$, which is the time of a complete vibration, and the difference $\frac{4D}{3B}\sqrt{\varkappa} + \ldots$. The effect of the resistance on the whole time involves, therefore, only the second and the higher powers of the coefficient of the resistance D; and it also disappears with the arc, as $\varkappa$, the square of the greatest velocity, becomes inconsiderable with respect to the velocity itself, and to the time $\frac{\pi}{\sqrt{B}}$.

THEOREM H. If $\frac{dds}{dt^2} + A\frac{ds}{dt} + Bs = 0$, d$t$ being constant, we have $e^{mt}(ds + asdt) = c$; m being $= \frac{1}{2}A \pm \sqrt{(\frac{1}{4}A^2 - B)}$, and $a = \frac{1}{2}A \mp \sqrt{(\frac{1}{4}A^2 - B)}$.

Demonstration. The fluxion of $e^{mt}(ds + asdt)$ is $e^{mt}(d^2s + adsdt + (mds + amsdt)dt) = e^{mt}(d^2s + (a+m)dsdt + amsdt^2)$; and, comparing this fluxion with the proposed equation, we have, for the coefficients, $a + m = A$, and $am = B$; whence $\frac{B}{m} + m = A$, $m^2 - Am = -B$, $m = \frac{1}{2}A \pm \sqrt{(\frac{1}{4}A^2 - B)}$, and $a = \frac{1}{2}A \mp \sqrt{(\frac{1}{4}A^2 - B)}$.

Example. Let the equation proposed be that of a cycloidal pendulum, vibrating with a resistance proportional to the velocity; that is, $\frac{dds}{dt^2} + A\frac{ds}{dt} + Bs = 0$.

Scholium 1. The resistance is here adequately expressed, in all cases, by the term $A\frac{ds}{dt}$, so that the equation is permanently applicable to the successive vibrations. Thus, in the second descent, on the negative side of the vertical line, Bs being negative, and $-s$ becoming nearer to 0, the fluxion ds is

positive, and $A\frac{ds}{dt}$ is of a contrary character to Bs, as it ought to be.

Solution. Since $m = \frac{1}{2}A \pm \sqrt{(\frac{1}{4}A^2 - B)}$, and $a = \frac{1}{2}A \mp \sqrt{(\frac{1}{4}A^2 - B)}$, it is obvious that the two radical quantities will be either possible or imaginary, accordingly as $\frac{1}{4}A^2$ is greater or less than B.

Case i. If A^2 is greater than $4B$, the resistance being very considerable, the solution becomes $e^{\frac{1}{2}At \pm \sqrt{(\frac{1}{4}A^2 - B)t}} \left(\frac{ds}{dt} + [\frac{1}{2}A \mp \sqrt{(\frac{1}{4}A^2 - B)}]\, s\right) = c$; and the velocity $v = -\frac{ds}{dt} = (\frac{1}{2}A - \sqrt{[\frac{1}{4}A^2 - B]})\, s - ce^{-\frac{1}{2}At - \sqrt{(\frac{1}{4}A^2 - B)}t}$ or $(\frac{1}{2}A + \sqrt{[\frac{1}{4}A^2 - B]})\, s - c'e^{-\frac{1}{2}At + \sqrt{(\frac{1}{4}A^2 - B)}t}$; and if the velocity be supposed to vanish when $s = \lambda$, and $t = 0$, we have $0 = \frac{1}{2}A\lambda - \sqrt{(\frac{1}{4}A^2 - B)}\lambda - c$ or $\frac{1}{2}A\lambda + \sqrt{(\frac{1}{4}A^2 - B)}\lambda - c'$.

Corollary 1. Hence it appears that such a pendulum would require an infinite time to descend to the lowest point, since the velocity cannot have a finite value when s vanishes, the exponential quantity never becoming negative.

Scholium 2. The coefficient B may also be written, for an actual pendulum, as measured in English feet, $\frac{32}{l}$, or $\frac{2g}{l}$, if we call g the descent of a falling body in the first second, which is, however, denoted, in the works of some authors, by $\frac{1}{2}g$, or even by $\frac{1}{4}g$. If we make $\frac{dds}{dt^2} + \frac{32}{l}\, s = 0$, when $s = l$, the force becomes such that $-d^2s = 32dt^2$, and $-\frac{ds}{dt} = 32$, which is the true velocity generated by such a force in a second of time. Supposing k to be the velocity with which the resistance would become equal to the weight, we must have, for $A\frac{ds}{dt}$, $\frac{32}{k}\frac{ds}{dt}$, in order that the force represented by A may become equal to that of gravity, and $A = \frac{2g}{k}$: and if h be the height from which a body must fall to gain the velocity k, since $h = \frac{k^2}{4g}$, $A^2 = \frac{4gg}{kk} = \frac{g}{h}$. Hence it follows, that when $A^2 = 4B$, which is the condition of the possibility of alternate

vibrations, $\frac{g}{h}=\frac{8g}{l}$, and $h=\frac{1}{8}l$, the resistance becoming equal to the weight when the body has fallen freely through one eighth of the length of the pendulum.

Case ii. Supposing now the resistance to be more moderate, and $\frac{1}{4}A^2$ to be less than B, and making $B-\frac{1}{4}A^2=C^2$, we shall have $\sqrt{(\frac{1}{4}A^2-B)}=\sqrt{(-C^2)}=\sqrt{-1}\,C$; the solution of the equation, $\frac{\mathrm{dd}s}{\mathrm{d}t^2}+A\frac{\mathrm{d}s}{\mathrm{d}t}+Bs=0$, will then be $\mathrm{d}\left(e^{\frac{1}{2}At\pm\sqrt{-1}Ct}\left[\frac{\mathrm{d}s}{\mathrm{d}t}+\frac{1}{2}As\mp\sqrt{-1}Cs\right]\right)=0$; whence, by taking the two different values in succession, and adding together their halves, we obtain $\mathrm{d}\left(e^{\frac{1}{2}At}\left[\frac{e^{\sqrt{-1}Ct}+e^{\sqrt{-1}Ct}}{2}(\mathrm{d}s+\frac{1}{2}As)+\frac{e^{-\sqrt{-1}Ct}-e^{\sqrt{-1}Ct}}{2}\sqrt{-1}Cs\right]\right)=o$; or, since $\sqrt{-1}=\frac{-1}{\sqrt{-1}}$,
$\frac{e^{\sqrt{-1}Ct}+e^{-\sqrt{-1}Ct}}{2}\left(\frac{\mathrm{d}s}{\mathrm{d}t}+\frac{1}{2}As\right)+\frac{e^{\sqrt{-1}Ct}-e^{-\sqrt{-1}Ct}}{2\sqrt{-1}}Cs=ce^{-\frac{1}{2}At}$.
Now the imaginary exponential quantities, thus combined, are the well-known expressions for the sine and cosine of the arc Ct (*Elem. Illustr.* § 358); and the last equation may be written thus, $\cos.\,Ct\left(\frac{\mathrm{d}s}{\mathrm{d}t}+\frac{1}{2}As\right)+\sin.\,Ct\;Cs=ce^{-\frac{1}{2}At}$; whence $v=-\frac{\mathrm{d}s}{\mathrm{d}t}=\frac{1}{2}As+\frac{\sin.\,Ct}{\cos.\,Ct}Cs-\frac{ce^{-\frac{1}{2}At}}{\cos.\,Ct}$. This fluent, if t were made to begin when $v=0$, would only afford us such expressions as have hitherto been found intractable; but nothing obliges us to limit the problem to this condition, and it is equally allowable to make the time t begin when $v=\frac{1}{2}As$, the corresponding value of s being called ς, then $\frac{1}{2}A\varsigma=v=\frac{1}{2}A\varsigma-c$; consequently $c=0$. The equation will then become $\frac{\mathrm{d}s}{s}+\frac{C\sin.\,Ct}{\cos.\,Ct}\mathrm{d}t+\frac{1}{2}A\mathrm{d}t=0$; whence $\mathrm{hl}\,s-\mathrm{hl}\cos.\,Ct=c'-\frac{1}{2}At=\mathrm{hl}\,\frac{s}{\cos.\,Ct}$, and $\frac{s}{\cos.\,Ct}=e^{c'-\frac{1}{2}At}$, or $s=\cos.\,Ct\,.\,e^{c'-\frac{1}{2}At}$; and when $t=0$, $s=e^{c'}=\varsigma$; consequently $\frac{s}{\varsigma}=\cos.\,Ct.\,e^{-\frac{1}{2}At}=\cos.\,Ct\left(1-\frac{1}{2}At+\frac{1}{8}A^2t^2-\frac{1}{48}A^3t^3+\ldots\right)$.
But, since $v\mathrm{d}t=-\mathrm{d}s=s\left(\frac{C\sin.\,Ct}{\cos.\,Ct}\,.\,\mathrm{d}t+\frac{1}{2}A\mathrm{d}t\right)$, it follows that v

must vanish whenever $\frac{C \sin. Ct}{\cos. Ct} + \frac{1}{2}A = 0$, or when $\tan Ct = \frac{-A}{2C}$, that is, in the first instance, very nearly when $Ct = \frac{-A}{2C}$, and $t = \frac{-A}{2CC}$, and $-\frac{1}{2}At = \frac{AA}{4CC}$, and $e^{-\frac{1}{2}At} = 1 + \frac{AA}{4CC}$, very nearly; so that, calling the primitive extent of the arc of vibration $s = \lambda$, we have $\frac{\lambda}{\varsigma} = \cos. Ct\left(1 + \frac{AA}{4CC}\right)$; $\cos. Ct$ being also, in this case, $= \sqrt{\left(1 - \frac{AA}{4CC}\right)} = 1 - \frac{AA}{8CC}$, and $\frac{\lambda}{\varsigma} = 1 + \frac{AA}{8CC}$, and $\frac{\varsigma}{\lambda} = \frac{8CC}{8CC+AA} = \frac{8B-2AA}{8B-AA} = 1 - \frac{AA}{8B-AA}$, corresponding to the versed sine of the time $\frac{-A}{2CC}$, or to the arc $\frac{-A}{2C}$, in the circle represented by Ct.

Corollary 2. It follows that both v and s must vanish continually at equal successive intervals, whenever $\tan Ct = \frac{-A}{2C}$, and when $\cos Ct = 0$, respectively; the descent, to the lowest point, will therefore occupy the time corresponding to $\frac{\pi}{4} + \frac{A}{2C}$, and the subsequent ascent to $\frac{\pi}{4} - \frac{A}{2C}$: the extent of the vibrations being always proportional to $e^{-\frac{1}{2}At}$.

Corollary 3. The greatest velocity must take place at the point where $A\frac{ds}{dt} + Bs = 0$, and $AC \tan Ct + \frac{1}{2}A^2 = B$, or $\tan Ct = \frac{B - \frac{1}{2}AA}{AC}$, and $\cot Ct = \frac{AC}{B - \frac{1}{2}AA}$, or, if we neglect A^2, $\cot Ct = \frac{A}{\sqrt{B}} = \cos. Ct = \frac{s}{\varsigma}$, very nearly.

Corollary 4. The diminution of the successive vibrations is expressed by the multiplier $e^{-\frac{1}{2}At}$, which, when $Ct = 2\pi$, the whole circumference, is $1 - \frac{A}{C}\pi$, and $\frac{A}{C}\pi\lambda$, or $\frac{A\pi\lambda}{\sqrt{B}}$ is the diminution of the value of s when the pendulum returns to the place from which it first set out, that is, the difference between the lengths of two vibrations, each corresponding to a semicircumference, and this difference is to $\frac{A}{\sqrt{B}}\varsigma$, or $\frac{A}{\sqrt{B}}\lambda$, the displacement of the point of greatest velocity, which

measures the greatest resistance, as π to 1, or as 3·1416 to 1. We have seen that, for a resistance varying as the square of the velocity, this proportion was as 8 to 3, or as 2·667 to 1.

Corollary 5. If the pendulum be supposed to vibrate in a second, the unity of time, the diminution of the arc 2λ in each vibration will be $\frac{1}{2}A \times 2\lambda$, and the successive lengths will vary as $e^{-\frac{1}{2}A}\, 2\lambda$, $e^{-A}\, 2\lambda$, and so forth: and after the number N of vibrations, the extent of the arc will be reduced from 2λ to $e^{-\frac{1}{2}NA}\, 2\lambda$; so that if we make $e^{-\frac{1}{2}NA} = M$, we have $\mathrm{hl}\, M = -\frac{1}{2}NA$, and $A = \frac{2}{N}\,\mathrm{hl}\,\frac{1}{M}$. Thus, if in an hour the vibrations were reduced to $\frac{2}{3}$ of their extent, which is rather more than appears to have happened in any of Captain Kater's experiments, we should have $N = 3600$, and $M = \frac{2}{3}$, whence $A = \frac{1}{1800} \times {\cdot}4054651 = {\cdot}00022526$, and $A^2 = {\cdot}000\,000\,050\,75$; and since $B = \frac{32}{l} = 9{\cdot}81$, $C = \sqrt{(B - \frac{1}{4}A^2)} = \sqrt{B}\sqrt{\left(1 - \frac{AA}{4B}\right)} = \sqrt{B}\left(1 - \frac{AA}{8B}\right) = \sqrt{B}\left(1 - \frac{AA}{78{\cdot}5}\right)$; the fraction being only $= .000\,000\,000\,65$; or about one second in 16 millions; that is in about 50 years.

Scholium 3. Although the isochronism of a pendulum, with a resistance proportional to the velocity, was demonstrated by Newton, yet Euler appears to have failed in his attempts to carry the theory of such vibrations to perfection; for he observes (*Mechan.* II. p. 312), *Etsi ex his appareat, tempora tam ascensuum quam descensuum inter se esse æqualia, tamen determinari non potest, quantum sit tempus sive descensuum sive ascensuum: neque etiam tempora descensuum et ascensuum inter se possunt comparari. Æquatio enim rationem inter* s *et* u *definiens ita est complicata, ut ex ea elementum temporis* $\frac{ds}{u}$, *per unicam variabilem non possit exprimi.*

Scholium 4. In confirmation of the solution that has been here proposed, it may not be superfluous to show the truth of the result in a different manner. Taking $s = se^{mt} \cos. Ct$,

we have $\frac{ds}{dt} = se^{mt}$ $(m \text{ cos. } Ct - C \text{ sin. } Ct)$, and $\frac{dds}{dt^2} = se^{mt}$ $(m^2 \text{ cos. } Ct - Cm \text{ sin } Ct - Cm \text{ sin. } Ct - C^2 \text{ cos. } Ct)$; whence $\frac{dds}{dt^2} + A\frac{ds}{dt} + Bs = se^{mt}$ $(m^2 \text{ cos. } Ct - 2Cm \text{ sin. } Ct - C^2 \text{ cos. } Ct + Am \text{ cos. } Ct - AC \text{ sin. } Ct + B \text{ cos. } Ct) = 0$, and $(m^2 - C^2 + Am + B) \text{ cos. } Ct - (2Cm + AC) \text{ sin. } Ct = 0$: an equation which is obviously true when the coefficients of both its terms vanish, and $2Cm = -AC$, or $m = \frac{1}{2}A$; and again $C^2 = m^2 + Am + B = \frac{1}{4}A^2 - \frac{1}{2}A^2 + B = B - \frac{1}{4}A^2$. The former mode of investigation is more general, and more strictly analytical; but this latter is of readier application in more complicated cases, and it will hereafter be further pursued.

Lemma. If a moveable body be actuated continually by a force equal to that which acts on a given pendulum, the body being in a state of rest when the pendulum is at the middle of its vibration, the space described in the time of a vibration will be to the length of the pendulum as the circumference of a circle is to its diameter. For the force being represented by cos. Ct, or cos. x, for the pendulum, it will become sin. x with regard to the beginning of the supposed motion, and the velocity, instead of sin. x, becomes $-$ cos. x, or $1 -$ cos. x; so that the space, instead of $1 -$ cos. x, is $x -$ sin. x, which, at the end of the semivibration, is $x = \frac{\pi}{2}$, instead of $1 - \text{cos. } x = 1$, the space described by the simple pendulum, which is equal to its length.

Scholium 5. There is a paradox in the relations of the diminution of the vibration to the distance measuring the greatest resistance, which it will be worth while to consider, in order to guard ourselves against the too hasty adoption of some methods of approximation which appear at first sight unexceptionable. The pendulum, if it set out from a state of rest at the point of greatest resistance, would perform a vibration to the extent of double the distance of that point, or $2\,\frac{A}{\surd B}\,\lambda$, the initial force being measured by that distance. Now, when the resistance is very small, its magnitude may be obtained without

sensible error from the velocity of the pendulum vibrating without resistance at the corresponding part of the arc; and the velocity may be supposed to vary as sin. Ct, and the resistance, in the case of this proposition, as sin. Ct or sin. x also. Hence it may be inferred, by means of the Lemma, that the whole diminution of the space will be to $\frac{A}{\surd B}\lambda$ as π to 1, or that it will be equal to $\frac{A}{\surd B}\pi\lambda$, which has already been found to be the actual difference of two successive semivibrations. The accuracy of this result, however, must depend on the mutual compensation of its errors; for the approximation supposes that, if the resistance vanished at the lowest point, the subsequent retardation would be such as to diminish the space by the effect of the diminution of the velocity acting uniformly through the remainder of the vibration, while in fact the diminution of the space from this cause would be simply equal to a part of the arc proportional to the diminution of the velocity; since the arc of ascent is simply as the velocity at the lowest point. Hence it is obvious, that the effects of the resistance are too much complicated with the progress of the vibration to allow us to calculate them separately; and, accordingly, when the resistance is as the square of the velocity, or as sin ^2x, the diminution of the velocity is expressed by $\frac{1}{2}x - \frac{1}{2}$ sin x cos x, and that of the space by $\frac{1}{4}x^2 - \frac{1}{4}$ sin. ^2x, which, at the end of a vibration, becomes $\frac{1}{4}\pi^2$ instead of π, that is, since the distance of the point of greatest velocity is here $s = D\lambda^2$, $\frac{1}{4}\pi^2 D\lambda^2 = 2{\cdot}467\ D\lambda^2$, while the more accurate mode of computation has shown that the true diminution of the space is $2{\cdot}667\ D\lambda^2$. (Theorem G.) If we chose to pursue the mode of approximation here suggested, with accuracy, it would be necessary to consider the resistance as a periodical force acting on a pendulum capable of a synchronous vibration, as hereafter in Theorem K, Schol. 1.

Theorem J. If $\frac{dds}{dt^2} + Bs + M$ sin. $Ft = 0$, we may satisfy the equation by taking $s =$ sin. $(\surd B\,.\,t) + \frac{M}{FF-B}$ sin. Ft.

Demonstration. The value of s here assigned gives us $\frac{ds}{dt} = \surd B$ cos. $\surd B.t + \frac{MF}{FF-B}$ cos. Ft, and $\frac{dds}{dt^2} = -B$ sin. $\surd B.t$ −

$\frac{MFF}{FF-B}$ sin. Ft; so that $\frac{dds}{dt^2} + Bs = B$ sin. $\sqrt{Bt} + \frac{MB}{FF-B}$ sin. $Ft - B$ sin. $\sqrt{Bt} - \frac{MFF}{FF-B}$ sin. $Ft = \frac{MB-MFF}{FF-B}$ sin. $Ft =$ $- M$ sin. Ft.

Corollary 1. If, in order to generalise this solution, we make $s = \alpha$ sin. $\sqrt{Bt} + \beta$ cos. $\sqrt{Bt} + \gamma$ sin. $Ft + \varepsilon$ cos. Ft, we may take any quantities at pleasure for α and β, according to the conditions of the particular case to be investigated; but ε must be $= o$; that is, the motion will always be compounded of two vibrations, the one dependent on the length of the pendulum, or on the time required for the free vibration, indicated by $\sqrt{Bt}$, the other synchronous with Ft, the period of the force denoted by M; the latter only being limited to the condition of beginning and ending with the periodical force.

Corollary 2. In the same manner it may be shown that the addition of any number of separate periodical forces, indicated by the terms M' sin. $F't$, M'' sin. $F''t$, . . ., will add to the solution the quantities $\frac{M'}{F'F'-B}$ sin. $F't$, $\frac{M''}{F''F''-B}$ sin. $F''t$, and so forth.

Example 1. Supposing a pendulum to be suspended on a vibrating centre, and to pass the vertical line at the same moment with the centre, we may make α and $\beta = 0$, and $s = \frac{M}{FF-B}$ sin. Ft only; the vibration being either direct or reversed accordingly as F is less or greater than $\sqrt{B}$, or than $\sqrt{\frac{32}{l}}$, which determines the spontaneous vibration of the pendulum.

Example 2. But if the ball of the pendulum be supposed to begin its motion at the moment that the centre of suspension passes the vertical line, we must make $s = \frac{M}{FF-B}(\sin Ft - \cos \sqrt{Bt})$; and the subsequent motion of the pendulum will then be represented by the sum of the sines of two unequal arcs in the same circle; and if these arcs are commensurate with each other, the vibration will ultimately acquire a double extent, and nearly disappear, in a continued succession of periods, provided that no resistance interfere. And the consequences of

any other initial conditions may be investigated in a manner nearly similar: thus, if the time of free vibration, under these circumstances, were $\frac{1}{3}$ of the periodical time, the free vibration, in which the motion must be supposed initially retrograde, in order to represent a state of rest by its combination with the fixed vibration, would have arrived at its greatest excursion forwards, after three semivibrations, at the same moment with the fixed vibration, and after three complete vibrations more would be at its greatest distance in the opposite direction, so as to increase every subsequent vibration equally on each side, and permanently to combine the whole extent of the separate arcs of vibration. But in this and in every other similar vibration, beginning from a state of rest in the vertical line—that is, at the point where the periodical force is evanescent—the effect of the free or subordinate vibration with respect to the place of the body will obviously disappear whenever an entire number of semivibrations have been performed.

Corollary 3. The paradox stated in the fourth scholium on the last theorem may be illustrated by means of this proposition, and will serve in its turn to justify the mode of computation here employed in a remarkable manner. It has been observed, in Nicholson's *Journal* for July 1813,* that the mode of investigating the effects of variable forces, by resolving them into parts represented by the sines of multiple arcs, and considering the vibrations derived from each term as independent in their progress, but united in their effects, may be applied to the problem of a pendulum vibrating with a resistance proportional to the square of the velocity; and that for this purpose the square of the sine may be represented by the series $\sin^2 x = .8484 \sin x - .1696 \sin 3x - .0244 \sin 5x - .00813 \sin 7x - .0029 \sin 9x - .0013 \sin 11x - \ldots$ Now, if we employ this series for resolving the resistance supposed in Theorem G into a number of independent forces, the greatest resistance being measured by $\frac{A}{\sqrt{B}}\lambda$, we shall have $.8484\,\frac{A}{\sqrt{B}}\lambda$ for the part supposed to be simply proportional to the velocity, whence, from Theorem H, we have $.8484\,\pi\,\frac{A}{\sqrt{B}}\lambda$ for the corresponding

* Supra, p. 270.

diminution of the vibration; that is, $2.6653 \frac{A}{\sqrt{B}} \lambda$. But it has been observed, in the preceding corollary, that the *place* of the pendulum will not be at all affected by any subordinate vibration after any entire number of complete semivibrations; and the slight effect of the *velocity* left in consequence of these subordinate vibrations may here be safely neglected, so that $2.6653 \frac{A}{\sqrt{B}} \lambda$ may be considered as the whole effect of the resistance with respect to the space described, which differs only by $\frac{1}{2000}$ of the whole from $2.666 \frac{A}{\sqrt{B}} \lambda$, the result of the more direct computation of Theorem G.

Scholium. An experimental illustration of the accuracy of the theorem may be found in the sympathetic vibrations of clocks, and in that of the inverted pendulum, invented by Mr. Hardy as a test of the steadiness of a support; for since the extent of the regular periodical vibration is measured by $\frac{M}{FF-B}$, it is evident that, however small the quantity of M may be, it will become very considerable when divided by $FF-B$, as F and $\sqrt{B}$ approach to each other; and accordingly it is observed, that when the inverted pendulum is well adjusted to the rate of a clock, there is no pillar so steady as not to communicate to it a very perceptible motion by its regular, though extremely minute, and otherwise imperceptible change of place.

Theorem K. In order to determine the effect of a periodical force, with a resistance proportional to the velocity, the equation, $\frac{dds}{dt^2} + A\frac{ds}{dt} + Bs + M \sin Gt = 0$, may be satisfied by taking $s = \alpha \sin Gt + \beta \cos Gt$, α being $= \frac{GG-B}{(GG-B)^2+AAGG} M$, and $\beta = \frac{AGM}{(GG-B)^2+AAGG}$; s being also $= \sqrt{(\alpha^2+\beta^2)} \sin \left(Gt + \text{arc tan} \frac{\beta}{\alpha}\right) = \frac{M}{\sqrt{([GG-B]^2+AAGG)}} \sin \left(Gt - \text{arc tan} \frac{AG}{B-GG}\right)$.

Since $s = \alpha \sin Gt + \beta \cos Gt$, $\frac{ds}{dt} = \alpha G \cos Gt - \beta G \sin Gt$, and $\frac{dds}{dt^2} = -\alpha G^2 \sin Gt - \beta G^2 \cos Gt = -G^2 s$; consequently

the equation becomes $(B-G^2)(\alpha \sin Gt + \beta \cos Gt) + \alpha AG \cos Gt - \beta AG \sin Gt + M \sin Gt = 0$, and $(B-G^2)\alpha - \beta AG + M = 0$, and $(B-G^2)\beta + \alpha AG = 0$; whence $\frac{\beta}{\alpha} = \frac{AG}{GG-B}$; also $\beta = \frac{M-(GG-B)\alpha}{AG} = \frac{\alpha AG}{GG-B}$ and $(G^2-B)M - (G^2-B)^2\alpha = \alpha A^2G^2$; consequently $\alpha = \frac{(GG-B)M}{(GG-B)^2+A^2G^2}$, and $\beta = \frac{AGM}{(GG-B)^2+A^2G^2}$. And since, in general, if $b = \tan \text{B}$, $\sin x + b \cos x = \sqrt{(1+b^2)}.\sin(x+\text{B})$; $\sin(x+\text{B})$ being $= \sin x \cos \text{B} + \sin \text{B} \cos x = \cos \text{B}(\sin x + \tan \text{B} \cos x)$, and therefore $\sin x + \tan \text{B} \cos x = \frac{\sin(x+\text{B})}{\cos \text{B}} = \sin(x+\text{B}) \sec \text{B} = \sin(x+\text{B})\sqrt{(1+b^2)}$: it follows that $\alpha \sin Gt + \beta \cos Gt = \alpha \sin\left(Gt + \text{arc}\tan\frac{\beta}{\alpha}\right)\sqrt{\left(1+\frac{\beta\beta}{\alpha\alpha}\right)}$; and $\alpha\sqrt{\left(1+\frac{\beta\beta}{\alpha\alpha}\right)} = \sqrt{(\alpha^2+\beta^2)} = \sqrt{\frac{(GG-B)^2+A^2G^2}{([GG-B]^2+A^2G^2)^2}} \times M = \frac{M}{\sqrt{([GG-B]^2+A^2G^2)}}$.

Corollary. If we put $M \cos Gt$ instead of $M \sin Gt$, we shall have $s = \alpha' \sin Gt + \beta' \cos Gt$; α' being $= \beta = \frac{AGM}{(GG-B)^2+A^2G^2)}$ and $\beta' = -\alpha = \frac{B-GG}{(GG-B)^2+AAGG}M$; and $s = \sqrt{(\alpha'^2+\beta'^2)} \sin\left(Gt + \text{arc}\tan\frac{\beta'}{\alpha'}\right) = \sqrt{(\alpha^2+\beta^2)}\sin\left(Gt + \text{arc}\tan\frac{B-GG}{AG}\right)$.

Scholium 1. Supposing B to approach very near to G^2, a case very likely to occur in nature because the effects which are produced, where it is found, will predominate over others, on account of the minuteness of the divisor; we may neglect the part of the denominator $(G^2-B)^2$, in comparison with A^2G^2, and the coefficient determining s will then become $\frac{M}{AG}$, the extent of the vibrations being inversely as A the coefficient of the resistance; and, indeed, when the whole force of the periodical vibration is expended in overcoming a resistance proportional to the velocity, it may naturally be imagined that the velocity should be inversely as the resistance. It follows also from the proposition, that in this case the arc tan $\frac{AG}{B-GG}$ approaching to a quadrant, the greatest excursions of the periodical motion and of the free vibration will differ

nearly one fourth of the time of a complete vibration from each other.

Scholium 2. Since s is a line, and B its numerical coefficient, making it represent a force, and since sin Gt is properly a number also, the coefficient M, both here and in Theorem J, must be supposed to include another linear coefficient, as μ, which converts the sine into a line, to be added to s, the distance from the middle point: that is, M must be considered as representing $B\mu$, in which μ is the true extent of the periodical change of the centre of suspension, and $B = \frac{2g}{l}$, as in other cases: so that M is $= \frac{2g}{l}\mu = 32\frac{\mu}{l}$, and $\mu = \frac{Ml}{2g} = \frac{1}{32}Ml$.

Corollary. In order to obtain a more general solution of the problem, we may combine the periodical motion thus determined with the free vibrations, as computed in Theorem H, the different motions, as well as the resistances, being totally independent of each other; but the most interesting cases are those which are simply periodical, the free vibration gradually diminishing, with the multiplier e^{-mt}, and ultimately disappearing.

THEOREM L. If there are several periodical forces, the equation $\frac{dds}{dt^2} + A\frac{ds}{dt} + Bs + M\sin Gt + N\sin Ft + \ldots = 0$, may be satisfied by taking $s = \alpha \sin Gt + \beta \cos Gt + \alpha' \sin Ft + \beta' \cos Ft + \ldots = \frac{M}{\sqrt{([G^2 - B]^2 + A^2G^2)}} \sin\left(Gt - \text{arc tan}\frac{AG}{B-GG}\right) + \frac{N}{\sqrt{([F-B]^2 + A^2F^2)}} \sin\left(Ft - \text{arc tan}\frac{AF}{B-FF}\right) + \ldots$

For, the equations expressing the space described being simply linear, the different motions and resistances are added or subtracted without any alteration of the respective relations and effects.

Scholium A free vibration may also be combined with this compound periodical vibration, by means of Theorem H; but it will gradually disappear by the effect of the resistance.

Lemma. For the addition of the arcs a and b, beginning with the well known equation $\sin(a \pm b) = \sin a \cos b \pm \sin b \cos a$, we have, by addition, $\sin(a+b) + \sin(a-b) = 2\sin a \cos b$, and

$\sin a \cos b = \frac{1}{2}\sin(a+b) + \frac{1}{2}\sin(a-b)$: then if $c = b + 90°$, $\cos b = \sin c$. whence $\sin a \sin c = \frac{1}{2}\sin(a + c - 90°) + \frac{1}{2}\sin(a - c + 90°)$: but $\sin(x+90°) = \cos x$ and $\sin(x-90°) = -\cos x$; consequently $\sin a \sin c = \frac{1}{2}\cos(a-c) - \frac{1}{2}\cos(a+c)$; again, if $c = a - 90°$, $\cos c = \sin a$, and $\cos c \cos b = \frac{1}{2}\sin(a+b) + \frac{1}{2}\sin(a-b) = \frac{1}{2}\sin(c + 90° + b) + \frac{1}{2}\sin(c + 90° - b) = \frac{1}{2}\cos(c+b) + \frac{1}{2}\cos(c-b)$. Also, since $\cos a \cos b = \frac{1}{2}\cos(a+b) + \frac{1}{2}\cos(a-b)$, and $\sin a \sin b = \frac{1}{2}\cos(a-b) - \frac{1}{2}\cos(a+b)$, we have, by subtraction, $\cos(a+b) = \cos a \cos b - \sin a \sin b$, and, by addition, $\cos(a-b) = \cos a \cos b + \sin a \sin b$.

Corollary. If $a + b = c$ and $a - b = d$, $\cos c + \cos d = 2\cos\frac{c+d}{2}\cos\frac{c-d}{2}$; and $\cos d - \cos c = 2\sin\frac{c+d}{2}\sin\frac{c-d}{2}$; also $\sin a + \sin b = 2\sin\frac{a+b}{2}\cos\frac{a-b}{2}$; and $\sin a - \sin b = \sin a + \sin(-b) = 2\sin\frac{a-b}{2}\cos\frac{a+b}{2}$.

THEOREM M. The equation, $\frac{dds}{dt^2} + A\frac{ds}{dt} + Bs + R\sin Ft \sin Gt = 0$ may be solved by taking $s = \alpha\sin([F-G]t+p) - \beta\sin([F+G]t+q)$; α being $\frac{\frac{1}{2}R}{\sqrt{([(F-G)^2-B]^2+A^2(F-G)^2)}}$, $\beta = \frac{\frac{1}{2}R}{\sqrt{([(F+G)^2-B]^2+A^2(F+G)^2)}}$, $p = \text{arc tan}\frac{B-(F-G)^2}{A(F-G)}$, and $q = \text{arc tan}\frac{B-(F+G)^2}{A(F+G)}$.

For since $\sin Ft \sin Gt = \frac{1}{2}\cos(F-G)t - \frac{1}{2}\cos(F+G)t$, the equation becomes $\frac{dds}{dt^2} + A\frac{ds}{dt} + Bs - \frac{1}{2}R\cos(F+G)t + \frac{1}{2}R\cos(F-G)t = 0$; whence we obtain the solution by comparison with Theorem K and its corollary.

SECT. IV.—*Astronomical Determination of the Periodical Forces which Act on the Sea or on a Lake.*

In order to compute, by means of the theory which has been laid down in the two preceding sections, the primitive tides of any sea or any portion of the ocean, we must compare its spontaneous oscillations with those of a narrow prismatic canal.

situated in a given direction with respect to the meridian, which in general must be that of the greatest length of the sea in question, neglecting altogether the actual breadth of the sea, which, if considerable, may require to have its own distinct vibrations compounded with those of the length, each being first computed independently of the other. Now, supposing the time required for the principal spontaneous oscillation of the sea or lake to be known, we must find the length of the synchronous pendulum, and taking $B = \frac{2g}{l} = \frac{32}{l}$, we must next find a series for expressing the force in terms of the sine, or cosines of multiple arcs, increasing uniformly with the time.

Now the force is measured, for the direction of the meridian of the spheroid of equilibrium, by sin z cos z, (Theorem A), z being either the zenith distance or the altitude: and it is obvious that, when the canal is situated obliquely with respect to the meridian of the spheroid, the inclination of the surface, and with it the force, will be diminished as the secant of the obliquity increases, or as the cosine of the obliquity diminishes; so that the force will vary as sin cos *Alt.* sin *Az.* if the canal be in an easterly and westerly direction; or if it deviate from that direction in a given angle, as sin cos *Alt.* sin (*Az.* + *Dev.*): and it is obvious that this force will vanish both when the luminary is in the horizon, and when it is in the vertical circle, perpendicular to the direction of the canal; that is, if we consider the force as acting horizontally on a particle at the middle of the length of the given canal; and the same force may be considered as acting vertically, with a proper reduction of its magnitude, at the end of the canal; for the horizontal oscillations at the middle must obviously follow the same laws as the vertical motions at the end.

The case, however, of a canal running east and west, admits a very remarkable simplification; and since it approaches nearly to that of an open ocean, which has been most commonly considered, it will be amply sufficient for the illustration of the present theory. For, in general, sin $Az. = \frac{\cos Decl. \sin Horary \angle}{\cos Alt.}$, and the expression, sin *Alt.* cos *Alt.* sin *Az.*, becomes in this case sin *Alt.* cos *Decl.* sin *Hor.* $\angle$. But sin *Alt.* = sin (*Lat.*)

sin *Decl.* + cos (*Lat.*) cos *Decl.* cos *Hor.* $<$, where (*Lat.*) is the latitude of the place, and calling sin (*Lat.*) for the given canal, L, and cos (*Lat.*), L′, the force becomes L sin *Decl.* cos *Decl.* sin *Hor.* $<$ + L′ cos^2 *Decl.* sin *Hor* $<$ cos *Hor.* $<$. Now, sin *Decl.* = cos *Obl. Ecl.* sin *Lat.* + sin *Obl. Ecl.* cos. *Lat.* sin *Long.*, where *Lat.* is the latitude of the luminary; and since $\cos \varphi = 1 - \frac{1}{2} \sin^2 \varphi + \frac{3}{8} \sin^4 \varphi - \frac{5}{16} \sin^6 \varphi + \ldots$, the true value of cos *Decl.* might be expressed, if required, by means of this series, and its three first terms would in general be sufficient for the computation.

But it will be more convenient to suppose the sun and moon to move in the ecliptic, and the ecliptic to be at the same time so little inclined to the equator, that the longitude may be substituted for the right ascension; a substitution which will cause but little alteration in the common phenomena of the tides. Then if the sun's longitude be ⊙, and the moon's ☽, the horary angles t and t', and the sine of the obliquity of the ecliptic α; we shall have sin *Decl.* = α sin ⊙, or α sin ☽; and cos *Decl.* $= 1 - \sin^2$ *Decl.* $+ \frac{3}{8} \sin^4$ *Decl.* $= 1 - \frac{1}{2} \alpha^2 \sin^2 \odot + \frac{3}{8} \alpha^4 \sin^4 \odot \ldots$, and sin *Decl.* cos *Decl.* $= \alpha \sin \odot - \frac{1}{2} \alpha^3 \sin^3 \odot + \frac{3}{8} \alpha^5 \sin^5 \odot \ldots$; also cos^2 *Decl.* $= 1 - \alpha^2 \sin^2 \odot$; whence the sun's force becomes $\text{L} \sin t \left(\alpha \sin \odot - \frac{1}{2} \alpha^3 \sin^3 \odot + \frac{3}{8} \alpha^5 \sin^5 \odot \ldots\right) + \text{L}' \frac{1}{2} \sin 2\, t \left(1 - \alpha^2 \sin^2 \odot\right) = \text{L} \sin t \left(\alpha \sin \odot - \frac{1}{2} \alpha^3 \left(\frac{3}{4} \sin \odot - \frac{1}{4} \sin 3 \odot\right) + \frac{3}{8} \alpha^5 \left[\frac{5}{8} \sin \odot - \frac{5}{16} \sin 3 \odot + \frac{1}{16} \sin 5 \odot\right]\right) + \frac{1}{2} \text{L}' \sin 2\, t \left(1 - \frac{1}{2} \alpha^2 + \frac{1}{2} \alpha^2 \cos 2 \odot\right) = \text{L} \sin t \left(\alpha' \sin \odot + \alpha'' \sin 3 \odot + \alpha''' \sin 5 \odot\right) + \left(\frac{1}{2} \text{L} - \frac{1}{4} \text{L}' \alpha^2\right) \sin 2\, t + \frac{1}{4} \text{L}' \alpha^2 \sin 2\, t \cos 2 \odot$; α' being $= \alpha - \frac{3}{8} \alpha^3 + \frac{15}{64} \alpha^5 \ldots$; or about .3645: $\alpha'' = \frac{1}{8} \alpha^5 - \frac{15}{128} \alpha^5 \ldots = .0078$, and $\alpha''' = \frac{3}{128} \alpha^5 \ldots = .00002$, and $\alpha^2 = .1585$. But $\sin t \sin \odot = \frac{1}{2} \cos (t - \odot) - \frac{1}{2} \cos (t + \odot)$, and $\sin 2\, t \cos 2 \odot = \frac{1}{2} \sin 2 (t + \odot) + \frac{1}{2} \sin 2 (t - \odot)$. Hence the sun's force becomes $S \left(\text{L}\, \alpha' \left[\frac{1}{2} \cos (t - \odot) - \frac{1}{2} \cos\right.\right.$

$(t + \odot)] + \text{L}\,\alpha'' [\frac{1}{2} \cos (t - 3 \odot) - \frac{1}{2} \cos (t + 3 \odot)] + \text{L}\,\alpha''' [\frac{1}{2} \cos (t - 5 \odot) - \frac{1}{2} \cos (t + 5 \odot)] + \frac{\text{L}'}{2} (1 - \frac{1}{2} \alpha^2) \sin 2\, t + \frac{\text{L}'}{4} \alpha^2 [\frac{1}{2} \sin 2 (t + \odot) + \frac{1}{2} \sin 2 (t - \odot)]\Big)$: and that of the moon may be expressed in the same manner, by substituting M, t' and ☽, for S, t, and $\odot$.

The effect of that part of the hydraulic resistance, which is proportional to the square of the velocity, must be expressed by an approximation deduced from the periodical character of the force, as depending on that of the primitive forces concerned; taking, however, the precaution to use such expressions only, as will always represent this resistance in its proper character as a retarding force: for if we simply found for it an equivalent expression, denoting accurately the square of the velocity, this square, being always positive, would imply a force acting always in the same direction. Now, we have already seen (Theorem J. Cor. 3),* that $\sin^2 x$ may be considered, with respect to its principal effect, as equivalent to $.8484 \sin x$: and, if we neglect, in the determination of the resistance, the effect of the smaller forces, and compute only that of the principal terms $\frac{1}{2}\,\text{L}' \sin 2t$, and $\frac{1}{2}\,\text{L}' \sin 2t'$, we may call the velocities depending on these forces $S' \cos (2t + s')$ and $M' \cos (2t' + m')$: S' and M' representing not exactly the proportion of the primitive forces of the sun and moon, but that of the tides depending on their combination with the conditions of the given sea or lake. The resistance will then be as the square of $S' [\cos (2t + s') + \cos (2t' + m')] + (M' - S') \cos (2t' + m')$: and when least, it will be $D (M' - S')^2$ and when greatest, $D (M' + S')^2$, the difference being $4DM'S'$; so that the difference may be sufficiently represented by $4\,D M'S' [\cos (2t + s') + \cos (2t' + m')] \times .8484$, or rather $\times (.8484)^2$, because the value of $\cos t + \cos t' = 2 \cos \frac{t + t'}{2} \cos \frac{t - t'}{2}$, which is to be squared, requires the reduction from 1 to .8484 for each of its factors; and in this manner we obtain a perfect representation of the period and quality of the resistance, and a very near approximation to its magnitude.

It will, however, be still more accurate to consider the resist-

* Supra, p. 321.

ance thus determined as comprehended in the value of the coefficient A, substituting for it, in the case of the solar tide, $A' = A + 2.88\ DM'$, and for the moon $A'' = A + 2.88\ DS' + .8484\ D\,(M' - S')$; this latter part expressing that portion of the resistance D which observes the period of the lunar tide, and which may therefore be considered as added to the resistance A for that tide only.

Hence, collecting all the forces concerned into a single equation, the expression will become $\frac{dds}{dt^2} + A'^{,''}\frac{ds}{dt} + Bs + S\Big(\textsc{l}\,\alpha'\,[\frac{1}{2}\cos(t - \odot) - \frac{1}{2}\cos(t + \odot) + \textsc{l}\,\alpha''\,[\frac{1}{2}\cos(t - 3\odot) - \frac{1}{2}\cos(t + 3\odot)] + \alpha'''\,[\frac{1}{2}\cos(t - 5\odot) - \frac{1}{2}\cos(t + 5\odot)] + \frac{\textsc{l}'}{2}(1 - \alpha^2)\sin 2t + \frac{\textsc{l}'}{4}\alpha^2\,[\frac{1}{2}\sin 2(t + \odot) + \frac{1}{2}\sin 2(t - \odot)]\Big) + M\Big(\textsc{l}\,\alpha'\,[\frac{1}{2}\cos(t' - ☽) - \frac{1}{2}\cos(t' + ☽)] + \textsc{l}\,\alpha''\,[\frac{1}{2}\cos(t - 3☽) - \frac{1}{2}\cos(t' + 3☽)] + \textsc{l}\,\alpha'''\,[\frac{1}{2}\cos(t' - 5☽) - \frac{1}{2}\cos(t' + 5☽)] + \frac{\textsc{l}'}{2}(1 - \alpha^2)\sin 2t' + \frac{\textsc{l}'}{4}\alpha^2\,[\frac{1}{2}\sin 2(t' + ☽) + \frac{1}{2}\sin 2(t' - ☽)]\Big) = 0$; and from each of these terms the value of the corresponding pair of terms in the value of s may be obtained independently, by comparison with the $M \sin Gt$ or $N \cos Gt$ of Theorem K, which gives us $\frac{(GG - B)\sin Gt + AG\cos Gt}{(GG - B)^2 + AAGG} M$, and $\frac{AG\sin Gt + (B - GG)\cos Gt}{(GG - B)^2 + AAGG} N$, respectively.

But without entering minutely into the effects of all the terms of the equation of the forces, it may be observed in general that their results, with regard to the space described, will not differ much from the proportion of the forces, except when their periods approach nearly to that of the spontaneous oscillation, represented by B. Thus since $\frac{1}{2}\cos(t - \odot) - \frac{1}{2}\cos(t + \odot)$ is the representative of $\sin t \sin \odot$, and since these terms will afford results in the form $\frac{1}{2}\alpha\cos(t - \odot) + \frac{1}{2}\beta\sin(t - \odot)$, and of $\frac{1}{2}\alpha'\cos(t + \odot) + \frac{1}{2}\beta'\sin(t + \odot)$; and if we neglected the slight difference of α and α', which is that of $\left(1 - \frac{\odot}{t}\right)^2 - B$, and $\left(1 + \frac{\odot}{t}\right)^2 - B$, $\frac{\odot}{t}$ being $\frac{1}{365.254}$ only, we should have

$\frac{1}{2}\,\alpha\,[\cos(t-\alpha)-\cos(t+\odot)] + \frac{1}{2}\beta\,[\sin(t-\odot)-\sin(t+\odot) =$ $\alpha\ \sin\ t\ \sin\ \odot + \beta\ \cos\ t\ \sin\ \odot = \sin\ \odot\ (\alpha\ \sin\ t + \beta\ \cos\ t)$; which is the same as if we considered the effect of the force sin t separately, and afterwards reduced it in the proportion of sin $\odot$. Hence it is obvious that for all modifications of the forces greatly exceeding in their periods the period of spontaneous oscillation, the effects may be computed as if the forces were exempt from those modifications, and then supposed to be varied in the same proportion as the forces; but we cannot be quite certain of the magnitude of the error thus introduced, unless we know the exact value of B, which determines the time of spontaneous oscillation.

Considering, therefore, in this simple point of view, the correct expression of the force L sin *Decl.* cos *Decl.* sin *Hor.* $<$ + L′ cos² *Decl.* sin *Hor.* $<$ cos *Hor.* $<$, or ½L sin 2 *Decl.* sin *Hor.* $<$ + ½L′ cos² *Decl.* sin 2 *Hor.* $<$: we may observe that the phenomena for each luminary will be arranged in two principal divisions; the most considerable being represented by ½L′ cos² *Decl.* sin 2 *Hor.* $<$, and giving a tide every twelve hours, which varies in magnitude as the square of the cosine of the declination varies, increasing and diminishing twice a year, being also proportional to the cosine of the latitude of the place, and disappearing for a sea situated at the pole: the second part is a diurnal tide, proportional to the sine of the latitude of the given canal, being greatest when the luminary is furthest from the equinox, and vanishing when its declination vanishes.

From these general principles, an attentive student may easily trace for himself the agreement of the theory here explained with the various modifications of the tides as they are actually observed, and as they are recorded by Lalande and Laplace, and as they are enumerated in the Article TIDES of the late editions of this 'Encyclopædia.' It remains, however, for us to inquire more particularly into the cause of the hitherto unintelligible fact, that the maximum of the spring tides in the most exposed situations is at least half a day, if not a whole day, later than the maximum of the moving forces

Now it is easy to perceive that since the resistance observing the lunar period is more considerable than that which affects

the solar tide, the lunar tide will be more retarded or accelerated than the solar; retarded when the oscillation is direct, or when $G^2 - B$ is positive, and accelerated when it is inverted, or when that quantity is negative: and that in order to obtain the perfect coincidence of the respective high waters, the moon must be further from the meridian of the place than the sun; so that the greatest direct tides ought to happen a little before the syzygies, and the greatest inverted tides a little after: and from this consideration, as well as from some others, it seems probable that the primitive tides, which affect most of our harbours, are rather inverted than direct.

If we wish to apply this theory with precision to the actual state of the solar and lunar motions, we must determine the value of the coefficients, from the tables of those luminaries: and first, making the unit of time a whole solar day, in which the horary angle t extends from 0° to 360°, the sun's mean longitude ☉ will be $\frac{t}{365.254}$ added to the longitude at the given epoch, and the moon's approximate horary angle t' will be found from the variation, or the moon's age in space.

Now, in Burckhardt's 'Tables,' p. 87, we find the variation for the midnight ending 1823, by adding the constant quantity 9° to the epoch for 1824. and (11ˢ. 14°. 44′. 44″) + 9° = 11ˢ.23°.44′.44″, or − (6°.15′.16″), according to the time of Paris: the movement for twelve hours is 6°. 5′. 43″; consequently, at noon, or 1824 Jan. 1. 0h, astronomical time at Paris, the variation is − (9′. 33″), corresponding to the movement of $18^{m}. 49^{s}$. in mean time, and the mean conjunction will take place at $18^{m}. 49^{s}$. Parisian time, which may be more compendiously expressed by calling it the true mean noon, in the time of the island of Guernsey or of Dorchester: and the movement in 24 hours being 12° 11′ 26.5″ = 12.19°, we shall have $t' = 360° - 12.19° = 347.81°$ when $t = 360°$, or $t' = \frac{347.81}{360} t = .96614t$; and the moon's horary angle, considered in relation to the circumference as unity, will always be $.96614t$, if t be the number of days elapsed, from the noon of the 1st Jan. 1814 at Guernsey.

The sun's mean longitude for the same epoch is (279°. 35′. 23.1″)=.77666, his longitude for any other time will therefore be .77666+.002738t=☉, and that of the moon, ☽ =.77666 +.03386t.

We may compute, with sufficient accuracy, the effect of the modifications produced by the change of the moon's distance, or the inequality of her motion in her orbit, or of the periodical change of the inclination of her orbit to the equator, which takes place from the revolution of the nodes, by simply considering the changes which will be produced in the forces concerned by these inequalities, and supposing the effects simply proportional to their causes. If, however, it were desired to determine these modifications with still greater precision, we might deduce approximate formulas for expressing them from the elements employed in the Tables.

The epoch of the moon's mean anomaly for 1824 is (4^s. 29°. 25′. 23.3″)+2′=151°.25′.23.3″; the movement for $12^h.18^m.49^s$. is (6°. 31′. 57″) + (9′. 47.9″) + 27″ = 6°. 42′. 12″, which gives 158°. 7′. 35″ for the mean anomaly at noon in the island of Guernsey. The daily movement being 13° 3.9′ = 13.065°, the mean anomaly will always be 158 127° + 13.065°t, reckoning t from the supposed epoch or day. The principal part of the central equation will then be, according to Burckhardt, 22692.4″ sin *An*., or (6°. 18.2′) sin (158⅛°+13.065°t), and its sine will be very nearly .11 sin (13.065°t+158.127°), which will represent the principal inequality of the longitude and of the variation, so that the variation, instead of 12.19°t, will become 12.19°t+6 3° sin (13.065°t+158.127°), and this subtracted from 360°t, leaves 347.81°t − 6.3° sin (13.065°t + 158 127°), the sine of which is nearly sin 347.81°t − cos 347.81°t .11 sin (13.065t+158.127°).

The equatorial parallax is nearly 57′ + 187″ cos *An*., or 57′ + 3.1′ cos (13.065°t +158.127°); and the disturbing force, which varies as the cube of the parallax, or of 57′ [1 + .0544 cos (13.065°t + 158.127°)] may be expressed, with sufficient accuracy, by 1 + .1632 cos (13.065°t + 158.127°).

The supplement of the node for 1824 is (2.10.56) + 2′ =

$70^\circ\ 58'$, to which we must add $(3'.10.6'')\ t$ for the time elapsed: and the longitude ☽ will be $279^\circ.\ 35'\ 23.1'' + (13^\circ.\ 10'.\ 35'')\ t$.

Although the value of the coefficient B is not directly discoverable, we may still obtain a tolerable estimate of its magnitude in particular cases, by inquiring into the consequences of assigning to it several different values, equal, for example, to the coefficient of the solar or lunar tide, or greater or less than either; while we assume also, for the coefficient of the resistance A, a great and a smaller value, for instance, $\frac{1}{3}$ and $\frac{1}{10}$, supposing D to be inconsiderable. We then find, from the expression $\sqrt{(\alpha^2+\beta^2)}\ M = \sqrt{(\alpha^2+\beta^2)}\ B\mu$ (Theorem J, Schol. 2) $= \frac{B(S,M)}{\sqrt{[(GG-B)^2+AAGG]}}$, for the solar tide, G being 1, if $B =$

$$B = \tfrac{1}{2},\quad .93442,\quad 1,\ \text{or}\ 4;$$

$$A = \begin{cases} \frac{1}{10}; & -.980,\quad -7.550,\quad 10,\quad 1.3324, \\ \frac{1}{3}; & -.832,\quad -2.742,\quad 3,\quad 1.3252; \end{cases}$$

and for the lunar, G being .96614, and

$$A = \begin{cases} \frac{1}{10}; & -1.122,\quad 10,\quad 8.197,\quad 1.3036, \\ \frac{1}{3}; & -\ .913,\quad 3,\quad 2.942,\quad 1.2968, \end{cases}$$

respectively.

Hence it appears that the resistance tends greatly to diminish the variation in the magnitude of the tides, dependent on their near approach to the period of spontaneous oscillation, and the more as the resistance is the more considerable: and supposing, with Laplace, that in the port of Brest, or elsewhere, the comparative magnitude of the tides is altered from the proportion of 5 to 2, which is that of the forces, to the proportion of 3 to 1; the multipliers of the solar and lunar tides being to each other as 5 to 6, we have the equation $\frac{36BB}{(1-B)^2+A^2} = \frac{25BB}{(n-B)^2+A^2}$, whence we find that B must be either .9380 or .6328; and the former value making the lunar tide only inverse, we must suppose the latter nearer the truth; and the magnitude of the tides will become 1.663 and 1.998: and it appears from the same equations, that n remaining $= .93442$, A cannot be greater than .632; and B would then be .78540: and if $A = 0$, the values of B would be .9617 or .6091. It

seems probable, however, that the primitive tides must be in a somewhat greater ratio than this of 2 to 1 and 5 to 3, when compared with the oscillations of the spheroid of equilibrium; and if we supposed $B = .9$ and A still $= \frac{1}{10}$, we should have 7.071 and 9.756 for their magnitude. Now if $B = .6328$, the tangents of the angular measures of the displacement, $\frac{\beta}{\alpha} = \frac{AG}{GG - B}$, becomes $\frac{1}{.3672}$ and $\frac{.96614}{.30160}$ respectively, giving us 69° 50′ and 72° 40′ for the angles themselves: and if $B = .9$, these angles become 45° and 70° 24′ respectively; the difference in the former case 2° 50′, and in the latter 25° 24′, which corresponds to a motion of more than twenty-four hours of the moon in her orbit.

It appears then that, *for this simple reason only*, if the supposed data were correct, the highest spring tides ought to be A DAY LATER than the conjunction and opposition of the luminaries; so that this consideration requires to be combined with that of the effect of a resistance proportional to the square of the velocity, which has already been shown to afford a more general explanation of the same phenomenon. There is indeed little doubt, that if we were provided with a sufficiently correct series of minutely accurate observations on the tides, made not merely with a view to the times of low and high water only, but rather to the heights at the intermediate times, we might form, by degrees, with the assistance of the theory contained in this article only, almost as perfect a set of tables for the motions of the ocean, as we have already obtained for those of the celestial bodies, which are the more immediate objects of the attention of the practical astronomer. There is some reason to hope, that a system of such observations will speedily be set on foot, by a public authority: and it will be necessary, in pursuing the calculation, on the other hand, to extend the formula for the forces to the case of a sea, performing its principal oscillations in a direction oblique to the meridian, as stated in the beginning of this section.

For such a sea, the calculations would be somewhat complicated, except in the case of its being situated at or near the

equator: we should then obtain, by proper reduction, for the volume of the force, putting D the sine of the duration, or of the angle formed by the length of the canal with the equator, and D′ its cosine, the expression D sin cos *Decl.* cos *Hor.* $<$ + D′ $\cos^2$ *Decl.* sin cos Hor. $<$: and the order of the phenomena would be less affected by the alteration of the situation of the canal than could easily have been supposed, without entering into the computation. This expression, when D = O, becomes, as it ought to do, identical with the former, making L = O

No. LVI.

EXTRACTS RELATING TO

THE THEORY OF THE TIDES.*

From Brande's Quarterly Journal of Science for 1824, vol. xvii. p. 295.

Of the objections which have been advanced by some authors against the established theory of the tides, the most material seems to be, that according to the Newtonian opinion, the moon must be supposed to repel the waters on the remoter side of the earth, instead of attracting them. The next is, that the lunar action must be sufficient to overcome the forces of gravity and cohesion. The third, that the time of high water is frequently three, and sometimes six hours later, than that of the moon's passage over the meridian.

"The difficulty in conceiving the apparent repulsion of the waters on the remoter side of the earth, which very naturally occurs to one who is but little conversant with the subject, appears to depend on a want of sufficient attention to the manner in which the mean solar and lunar attractions are counterbalanced. We are unconsciously disposed to consider the earth, especially in comparison with the moon, as a body perfectly at rest, or at most as an immense sphere poised on its axis, or having some secret support connected with its centre. And it is true, that if the earth were suspended as an apple hangs on

* The portions of these extracts which are marked by inverted commas are taken from a review, by the author, of a 'New Theory of the Tides,' by Ross Cuthbert, Esq., in the Quarterly Review for October, 1811, a work not otherwise entitled to notice than as expressing with tolerable precision of language the popular objections to the received theory of the tides. This review gave not merely an answer to these and similar objections, but likewise an exposition of the additions which the theory had received from the researches of Laplace and Dr. Young at the period at which it was written, and which were subsequently published in Nicholson's Journal for 1813, forming No. LIV. of this volume.—*Note by the Editor.*

a stalk, or a terrestrial globe on the pins which connect it with the brazen meridian, the attractive force of a distant body would necessarily tend to collect a fluid surrounding it, about the part nearest to the disturbing body. But in fact the force counteracting the solar and lunar attraction is by no means to be confounded with the operation of a support of any kind, attached to the solid parts of the sphere alone; for the force actually concerned in this case is equally efficacious with respect to the fluid parts; and, acting exactly alike on every particle of the earth and sea, it precisely counterbalances the mean force of attraction, and leaves only the difference of the attractive powers, which are different for the different parts of the earth and sea, to exhibit its effects in disturbing the relative situations of those parts. This counterbalancing power is well known under the name of the centrifugal force, being derived from the velocity of the earth, either in its annual revolution round the sun, or, in the case of the moon, from its velocity in revolving round the common centre of inertia of the earth and moon. Since the earth actually falls at every instant as much within the tangent of its annual orbit, or the temporary line of direction of its motion, as it would descend towards the sun in an equal time, if it were otherwise at rest, this change of relation of the revolving body, which prevents its actual approach to the centre of attraction, and counteracts the force of gravitation, is, not improperly, considered as constituting a distinct force, and is characterized by the term centrifugal. Before the introduction of the Newtonian theory, an attempt was made by the celebrated Dr. Wallis, to deduce the tides from a difference of the centrifugal forces affecting the opposite parts of the earth and sea, in revolving round the sun, and round the common centre of gravity of the earth and moon; and Mr. Ferguson, in later times, has endeavoured to explain an opinion of a similar nature, by means of the whirling table; but the apparatus of Ferguson was so constructed, as to produce a greater velocity of rotation in the remoter than in the nearer parts of the revolving system of bodies, which is a difference that does not exist in the case to be investigated; for the velocity of the different parts of the earth and sea, with respect to their common annual

revolution round the sun, is precisely the same, the diurnal rotation being altogether independent of this revolution, and producing modifications of force, which have their separate compensations, as distinctly indeed as the monthly revolution of the moon, which does not affect the velocity of its mean annual revolution round the sun. together with the earth.

" It is therefore so far from being true, that the inequality of the centrifugal force, at different parts, gives rise to any part of the phenomena of the tides, that, on the contrary, the perfect uniformity of this force is the basis of the determination of the powers immediately concerned in these phenomena. The Atlantic and the Pacific oceans are subjected to a centrifugal force precisely equal to that which affects the solid parts of the earth ; but when the luminary is over the Atlantic, its attraction for that ocean is greater than for the central part, and consequently greater than the centrifugal force, so that this differential attraction tends to elevate the Atlantic; at the same time that its attraction for the Pacific Ocean is less than the mean attraction, and less than the centrifugal force, which therefore prevails over the attraction, and the differential force tends to raise the Pacific Ocean almost as much as it tends to raise the Atlantic in the opposite direction.

" There is also an additional force, derived from the obliquity of the action of the luminary on the parts of the earth not immediately below it, which tends to compress the lateral parts, and to increase the elevation at the ends of the diameter pointing to the luminary.

" The readiest way of calculating the operation of all these forces is, to reduce them to a horizontal direction, and to determine what inclination of each part of the surface of the sea, considered as an inclined plane, will cause such a tendency, in a particle situated on it, to move in a contrary direction, as precisely to counterbalance, not only these forces, but also the new disturbing force, derived from the attraction of the parts thus elevated ; and it may easily be shown, that all these conditions will be fulfilled, if we attribute to the surface of the sea the form of an oblong elliptic spheroid, differing but little from a sphere."

Now, "we have only to recollect, with respect to the first objection" already mentioned, "that we are by no means required to imagine that the moon repels the remoter parts of the earth and sea; but merely to understand, that these parts are left a little behind, while the central parts fall more within the tangent towards the moon, and the nearer parts still more than the central parts: nor is this a fact of which our belief must rest on any observed phenomena of the tides, since it is completely demonstrable from the general laws of gravitation and of central forces; so that if no such tides were under any circumstances observable, their non-existence would afford an unanswerable argument against the universality and accuracy of these laws, as they are inferred from other phenomena.

"The second objection is already answered in the statement of the mode of operation of the disturbing force. The action of this force is only supposed to be sufficient to retain a particle of water in equilibrium on a surface of which the inclination to the horizon is scarcely perceptible, or to cause the whole gravitation of a column four thousand miles in height, immediately under the luminary, to be equal only to the gravitation of a column shorter by a few inches or feet, in another part of the spheroid. The objectors have confounded this very slight modification of the force of gravitation, with its complete annihilation by a greater force: and with respect to the force of cohesion, it is so little concerned in counteracting any elevation of this kind, that to attempt to calculate the magnitude of any resistance derived from it would be perfectly ridiculous.

"The third objection is only so far more valid, as it is opposed to the imperfect and superficial notions," which some authors have entertained, "of the supposed operation of the forces concerned:" as if the sea could instantly accommodate itself to the temporary form which would afford an equilibrium. In fact, however, it is just as likely to happen, in the open ocean, that the transit of the luminary may coincide with the time of low water as with that of high water; and in more limited seas and lakes, there is no hour in the twenty-four at which high water may not naturally be expected to take place, according to the different breadth and depth of the waters con-

cerned; while under other circumstances, it may happen to be high water only once a day, or once a fortnight, or there may be no tide at all, without any deviation from the strictest regularity in the operation of the causes concerned. Since the subject has hitherto been considered as extremely intricate, and has not indeed yet been freed from all its embarrassments, we shall here endeavour to explain the principles on which the investigation has been conducted.

"The attempts that were made by Newton, to compute the effects of the solar and lunar attraction on the sea, went no further than to the determination of the magnitude of the elevation which would at any given time afford a temporary equilibrium: and even Maclaurin was satisfied with having ascertained the precise nature of the form which the waters must assume in such a case. But it is obvious that these determinations are by no means sufficient for ascertaining the motions which arise from the change of relative situation induced by the earth's rotation, since the form, thus ascertained, only affords us a measure of the force by which the waters are urged, when they do not accord with it, and by no means enables us to say, without further calculation, how nearly they will at any time approach to it. In fact, the change of the conditions of equilibrium determines only the magnitude of this force, such as it would be if the sea remained at rest, while it is in reality materially modified, at any given time, by the effect of the motions which have previously taken place: and supposing its true magnitude to be ascertained, its immediate operation will at all times be complicated with the conditions under which an impulse of any kind is capable of being communicated to the neighbouring parts of the sea, which depend on the depth of the sea, as well as on the form of the earth.

"Mr. Laplace has undertaken the investigation of the theory of the tides, with all these additional complications; and he considers it as constituting without exception the most difficult department of the whole science of astronomy; and yet this consummate mathematician has omitted to include in his calculation the effects to be attributed to resistances of various kinds, and to the irregularities of the form of the sea, which appear

to us to constitute by far the more material difficulties in the inquiry. The general problem, relating to the oscillation of a fluid completely covering a sphere, and moving with little or no resistance, which Mr. Laplace has solved by a very intricate analysis, is capable of being exhibited in a much less embarrassed, and, we apprehend, even in a more accurate manner, by a mode of investigation, which is equally applicable to the tides of narrow seas and of lakes, and which may easily be made to afford a correct determination of the effects of resistance, as well as a ready mode of discovering the laws of motions governed by periodical forces of any kind; at least so far as these forces are capable of being represented by any combinations of the sines of arcs, which increase uniformly with the time.

"The essential character of this method consists in comparing the body actuated by the given force to a pendulum, of which the point of suspension is caused to vibrate regularly to a certain small extent: the length of the pendulum being supposed to be such as to afford vibrations of equal frequency with the spontaneous vibrations of the moveable body, and the point of suspension to be carried by a rod of such a length as to afford vibrations of equal frequency with the periodical alternations of the force. It is then shown that such a pendulum may perform regular vibrations, contemporary with the alternations of the periodical force, and inversely proportional in their extent to the difference between the length of the two rods: and that, whatever may have been the initial state of the pendulum, the motion thus determined may be considered as affording a mean place, about which it will at first perform simple and regular oscillations; but that a very small resistance will ultimately cause these to disappear:" so that the particular solution of the problem, which indicates a series of vibrations as they *may* be performed, is thus rendered general; since every other initial state of the vibrations *must* ultimately terminate in this series.

"Now the sea, or any of its portions, may be considered as bodies susceptible of spontaneous vibrations, precisely similar to the small vibrations of a pendulum; and the semidiurnal

variation of the form which would afford an equilibrium, in consequence of the solar and lunar attractions, is perfectly analogous to the regular vibration attributed to the point of suspension of the pendulum. The frequency of the simple oscillations of the sea, or of any of its parts, supposing their depth and extent known," and their form sufficiently simple, " may easily be deduced from the important theorem of Lagrange, by which the velocity of a wave of any kind," when sufficiently broad, " is shown to be equal to the velocity of a heavy body, which has fallen through half the height of the fluid concerned: but in the case of a tide extending to any considerable portion of the surface of the globe, this velocity must be somewhat modified according to the comparative density of the central and the superficial parts.

" The most remarkable consequence of this analogy is the law, that if the simple oscillations, of which the moving body is susceptible, be more frequent than the period of the recurring force, the pendulum will follow its point of suspension with a direct motion; but if the spontaneous vibrations be the slower, the motions will be inverted with respect to each other: and with regard to the tides, we may infer from this mode of calculation, that supposing the earth to be between five and six times as dense as the sea, the oscillations of an open ocean can only be direct, if its depth in the neighbourhood of the equator be greater than fifteen or sixteen miles; and that if the depth be smaller than this, the tides must be inverted, the time of low water corresponding, in this case, to the transit of the luminary over the meridian.

" This distinction has not been explicitly made by Mr. Laplace, although he has calculated, that for a certain depth, of a few miles only, the tides of the open ocean must be inverted, and that for greater depths they will be direct: but the intricacy of his formulæ seems to render their use laborious, and perhaps liable to some inaccuracy; and in the application of his theory, he seems to have lost sight even of the possibility of an inverted tide. In narrower seas, which Mr. Laplace has not considered, a smaller depth will constitute the limit between these two species of tides; and in either case the approach of the

depth to this limit will be favourable to the magnitude of the tide." It may also be remarked, that if the depth of the sea became gradually smaller in receding from the equator, till it vanished at the poles, its surface, as well as that of the earth, having the form of an oblate spheroid, the time required for a wave to travel round it would be equal in all latitudes, and the tides would be of the same species in every part: while the tides of the atmosphere, on the other hand, independently of the resistance, would be indirect at the equator, and direct near the poles.

"However the primitive oscillation may be constituted, it is easy to understand, that it will be propagated through a limited channel, connected with the main ocean, in a longer or shorter time, according to the length and depth of the channel; and that if the channel be open at both ends, the tide will arrive at any part within it by two different paths; and the effects of two successive tides may in this manner be so combined as to alter very materially the usual course of the phenomena: for instance, if there were about six hours' difference in the times occupied in the passage of the two tides over their respective paths, the time of the high water belonging to one tide would coincide with that of the low water belonging to the other, and the whole variation of the height might in this manner be destroyed, as Newton has long ago observed with respect to the port of Batsha: and it may be either for a similar reason, or from some other local peculiarity of situation, that no considerable tides are observed in the West Indies; if indeed it is true, that the tides are so much smaller there than might be expected from calculation; for in fact the original tides of an open sea, not exceeding a mile or two in depth, would amount to a few inches only, even without allowing for the effects of resistance. In the middle of a lake, or of a narrow sea, there can be little or no primitive elevation or depression; and the time of high water on its shores must always be about six hours before or after the passage of the luminary over the middle; so that from this source we may derive an infinite diversity in the times at which these vicissitudes occur in different parts.

"The effects of resistances of various kinds, in modifying the time of high water, cannot easily be determined in a direct and positive manner from immediate observation. Mr. Laplace appears to be of opinion that these resistances are wholly inconsiderable; but if any dependence can be placed on the calculations of Dubuat, we ought to expect a very different result, since, according to Dubuat's formula, the resistance, in the case of a tide of any moderate magnitude, must far exceed the moving power. From this result, however, nothing can be concluded with certainty, except that the formula is extremely defective with respect to great depths and slow motions; yet we may infer from it, as a probable conjecture, that the resistance must be great enough to produce some perceptible effects, and even that it must be greater than would be expected from another mode of calculation founded on the same experiments, (*Phil. Trans* 1808;* *Suppl. Enc. Br.*, Art. HYDRAULICS,) which would give the proportion of the greatest resistance to the greatest moving force only as $\frac{1}{3}$ of the height of the tide, increased by about ten feet, to the whole depth of the ocean concerned, at least on the supposition of a uniform depth and a smooth bottom, which indeed must be far from the truth; since the inequalities of the bottom of the sea must tend very greatly to increase the resistance, especially that part of it which varies as the square of the velocity.

"Now it has been demonstrated (*Nich. Journ.*;† *Illustr. Cel. Mech.*; *Suppl. E. Br.*, Art. TIDES)‡ that a resistance simply proportional to the velocity, would not disturb the perfect regularity of the oscillations concerned, and that it would only retard them when direct, and accelerate them when inverted, by the time corresponding" to a certain arc, of which the tangent is to the radius, as the velocity due to half the length of the pendulum synchronous with the periodical force is to the velocity at which the resistance becomes equal to the force of gravity, and as the length of the pendulum synchronous with the spontaneous oscillation to the difference of the lengths of these two pendulums conjointly. "Nor will the displacement produced by an equal mean resistance, varying as the square of

* Supra, No. XXIV., Vol. I., p. 497. † Supra, p. 266. ‡ Supra, p. 322.

the velocity, be materially different; the body or surface merely oscillating a little about its mean place, in consequence of the different distribution of the resistance.

"Here, then, we have another source of very great diversities in the times of the tides, according to the dimensions of the seas concerned, even in those parts in which the tides may be supposed to be rather original than derivative, not excepting the most widely extended oceans. There are, however, other considerations, which limit, in some measure, the probable magnitude of a resistance, varying either accurately or very nearly in proportion to the square of the velocity; and the chief of these is the time of high water at the spring and neap tides, which must be very differently affected by such a resistance, since it must necessarily cause a much greater acceleration or retardation of the spring tides than of the neap tides. Hitherto it has only been observed that, in particular ports, the greatest tides have happened the earliest; but no accurate comparisons of the times of high and low water have been made in a sufficient variety of circumstances to authorise our forming any general conclusion of this kind. It might indeed be supposed, that this diversity of the relative time of high water might be modified and concealed by a difference of velocity in the progress of the different tides from their source in the ocean to the places of observation, according to the different degrees of resistance opposed to them: but, if we can depend on a mode of calculation which has occurred to us, the velocity with which a wave or tide is propagated, is not materially affected by a resistance of any kind, its magnitude only being gradually reduced, and even its form remaining little altered by this cause, when the resistance is nearly proportional to the velocity;" although, as the form of a wave is evidently altered in approaching the shore, its summit advancing more rapidly than its basis, till it falls over and the wave breaks, so a tide remote from the ocean is generally observed to rise somewhat more rapidly than it falls.

"Another limitation of the magnitude of a resistance, varying as the square of the velocity, is the modification of the apparent proportion of the solar to the lunar force, which must arise from it. In assuming that the comparative magnitudes of the tides

in the open sea must be precisely the same with those of the disturbing forces which occasion them, astronomers have hitherto neglected two very material circumstances; one, the effect that a greater approach of the frequency of the spontaneous oscillations, to the solar or lunar period, must have in augmenting the respective tide; the other, the greater diminution of the spring than of the neap tides by the operation of a resistance proportional to the square of the velocity, which gives to the lunar tide a greater apparent preponderance. Mr. Laplace is obliged to have recourse to some imaginary peculiarities in the local situation of the port of Brest, in order to explain the existence of lunar and solar tides in the proportion of three to one, while the other phenomena, depending on the moon's attraction, make it improbable that the lunar force can be to the solar in a much greater ratio than that of five to two. But, in fact, the proportions of the tides in other ports, very differently situated, for instance at St. Helena, are nearly the same with those which have been observed at Brest; and it is demonstrable, that such a diminution of the apparent solar force must necessarily be the consequence of the operation of any resistance, proportional to the square of the velocity; besides being in part dependent, according to the most probable suppositions, upon the actual depth of the sea, as being more favourable to the exhibition of a lunar than a solar tide.

"There remains to be explained the interval which elapses between the time of new or full moon, and the occurrence of the highest tides, amounting at Brest to about a day and a half, and at London Bridge probably to two days. The most simple supposition respecting this interval, is that which Mr. Laplace has adopted; as the retardation is greater at London Bridge than at Brest, so it may be imagined that there are other places, still more exposed than Brest to the great oceans, at which it will altogether disappear. We cannot, however, discover anything like a progressive succession of this kind in the tides which are observed at different parts of the continent; nor would so great a time as a day and a half be required for the passage of a tide over more than half the circumference of the globe, upon any probable estimate of the depth of the sea."

The full development of the manner, in which the resistance may be supposed to cause this retardation, will be found in the Supplement to the Encyclopædia.

"We have assigned abundant reasons for the diversity which occurs in the time of high water at any given period of the moon's revolution in places differently situated; and this time being once ascertained for any one tide, we may easily infer by calculation the time at which every other tide will occur; and we shall find in this sequence the most perfect coincidence between theory and observation. Thus, if the high water of the spring tides, derived from the coincidence of the solar and lunar high waters, soon after the new or full moon, happened at any port precisely at noon, the next time of the high water belonging to the solar tide would of course be at midnight, and that of the lunar high water twenty-five minutes later; and the true time of high water will divide this interval nearly in proportion to the apparent forces, and will occur about eighteen minutes after midnight," [the interval being $12^h\ 18^m$, and not $12\frac{1}{2}^h$, as it has been hastily assumed for the table of the Supplement:] "and the next day it will be high water about thirty-six minutes after twelve. This retardation will increase from day to day, since its mean daily value is about fifty minutes; and at the neap tides following the moon's quadratures, it will become about twice as great as at the syzygies, its different values, in these cases, being nearly proportional to the magnitude of the spring and neap tides; so that Bernoulli has considered them as affording the most correct estimate of the comparative magnitude of the solar and lunar forces; although they are probably less capable of being accurately determined by direct observation than the different elevations and depressions. We can scarcely imagine it possible that any individual, acquainted with these simple facts alone, to say nothing of many others, equally well established, could for a moment entertain the slightest doubt of the real and immediate dependence of the tides on a combination of the solar and lunar attractions."

"In the diurnal and annual variations of the height of the tides, there is no peculiar difficulty. The declinations and distances of the luminaries modify their forces in a manner which

is easily determined; and the periods of these changes being much greater than the times of spontaneous oscillation in any of the seas concerned, the effects directly follow their causes, almost in the simple proportion of the intensity of the forces concerned. Mr. Laplace has calculated, that in an ocean of equable depth, the difference between the heights of the morning and evening tides, depending on the declination of the luminary, must wholly disappear; but we cannot help suspecting that there must be an imperfection in some of the many steps of his investigation. The depth would be equable if the whole sphere were fluid; and it will not be denied that in this case there would be no difference in the morning and evening tides, very nearly coinciding with that of the primitive variations of the figure affording an equilibrium: nor can we discover any imperfection in the method, which Mr. Laplace himself has sometimes adopted, of considering the difference of the two tides as a separate diurnal tide, and determining its magnitude precisely in the same manner as if it existed alone."

"When a regular tide moves continually forwards in an open ocean, the progressive motion of the fluid is the greatest, or in other words, the flood is the strongest, where the elevation is greatest, and the motion is retrograde, constituting the ebb, wherever there is a depression. In a river, the effect of a stream would only so far modify the velocity, as to make it proportional to the elevation above or the depression below a different level; but if a river or channel of any kind terminated abruptly, so as to cause a reflection, the progressive velocity would commence from the time of low water, and continue till that of high water only, or even be counteracted by the motion of the current, so as to cease still earlier, and to commence later. The rivers, in which our tides are commonly observed, seem to hold a middle place between these two cases: at Lambeth, for instance, the flow of the tide is continued, not during the whole time that the water remains elevated above a certain level, but about three quarters of an hour after the time of high water, at which it would cease near the end of a channel terminating abruptly. And it is probable that by similar con-

siderations the course of the ebb and flood tides might be explained in many other cases."

"If we apply the same mode of calculation to the tides of the atmosphere, they will appear to be subject to some very singular modifications. At the poles they must be very small; at the equator moderate; but at the latitude of about 42°, where the rotatory velocity of the earth's surface is equal to the velocity with which any impression is transmitted by the atmosphere, or at about 40° of the lunar tide, the height of the oscillations will only be limited by the resistances, the greatest elevation occurring about three hours after the transit of the luminary; nearer the pole they will occur earlier than this, and nearer the equator a little later." Possibly, indeed, the slight obliquity in the direction of the high water might have some little tendency to equalize the height of the tides of different parts of the atmosphere: "it seems, however, to be a mistake to suppose, that the effects of the atmospherical tides must be more perceptible near the equator than in temperate climates; and the variations of the barometer which have been observed between the tropics, are manifestly independent of the lunar attraction, occurring regularly at certain hours of the day or night; as indeed the tides of the ocean might have been expected to occur, if they had really been derived from the" meteorological causes to which some authors have "chosen to attribute them."

Of the article TIDES in the *Supplement*, the first section relates to the "Progress of contemporary tides as inferred from the times of high water in different ports." The author's conclusions from a tabular comparison of observations are these:—*

"First, that the line of contemporary tides is seldom in the exact direction of the meridian, as it is supposed to be universally in the theory of Newton and of Laplace; except, perhaps, the line of the twenty-first hour [of Greenwich time] in the Indian Ocean, which appears to extend from Socotora to the Almirantes, and the Isle of Bourbon, lying nearly in the same longitude.

"Secondly, that the southern extremity of the line advances

* Supra, p. 297.

as it passes the Cape of Good Hope, so that it turns up towards the Atlantic, which it enters obliquely, so as to arrive, nearly at the same moment, at the Island of Ascension, and at the Island of Martin Vaz, or of the Trinity.

"Thirdly, after several irregularities about the Cape Verd Islands, and in the West Indies, the line appears to run nearly east and west from St. Domingo to Cape Blanco, the tides proceeding due northwards; and then, turning still more to the right, the line seems to become N.W. and S.E. till at last the tide runs almost due east up the British Channel, [while another part of it passes] round the north of Scotland into the Northern Ocean, sending off a branch down the North Sea to meet the succeeding tide at the mouth of the Thames.

"Fourthly, towards Cape Horn again there is a good deal of irregularity; the hour lines are much compressed between South Georgia and Tierra del Fuego, perhaps on account of the shallower water about the Falkland Islands and South Shetland.

"In the fifth place, at the entrance of the Pacific Ocean, the tides seem to advance very rapidly to New Zealand and Easter Island; but here it appears to be uncertain whether the line of contemporary tides should be drawn nearly north and south from the Gallapagos to Tierra del Fuego, or N.E. and S.W. from Easter Island to New Zealand; or whether both these partial directions are correct: but on each side of this line there are great irregularities, and many more observations are wanting before the progress of the tide can be traced, with any tolerable accuracy, among the multitudinous islands of the Pacific Ocean, where it might have been hoped that the phenomena would have been observed in their greatest simplicity, and in their most genuine form.

"Lastly, of the Indian Ocean the northern parts exhibit great irregularities, and among the rest they afford the singular phenomenon observed by Halley in the port of Tonkin, and explained by Newton in the *Principia:* the southern parts are only remarkable for having the hour lines of contemporary tides considerably crowded between New Holland and the Cape of Good Hope, as if the seas of those parts were shallower than elsewhere."

The second section relates to the "disturbing forces that occasion the tides," and presents nothing that is not readily demonstrable, and indeed universally admitted, except, perhaps, the magnitude of the primitive elevation, produced by the lunar and solar forces, which is made two feet and ten inches respectively, or at the very utmost 2¼ feet and eleven inches, for the actual density of the earth and sea, instead of the much greater height commonly assigned to it, on the very erroneous supposition of a homogeneous sphere of water.

The third section investigates the "effects of resistance in vibratory motions, whether simple or compound," and reduces into a somewhat more technical or fashionable form the propositions which the author had before deduced from a geometrical mode of representation, but with considerable extensions and improvements: and as a corollary tending to illustrate the accuracy of his formulæ, he has applied them to the problem of a pendulum moving with a resistance proportional to the velocity, which had been left incomplete by Euler. He has shown that the resistance, in Captain Kater's experiments, could only have caused an error of a second in about fifty years: a quantity certainly altogether insignificant, but which could not with propriety be wholly neglected, while it was known that its magnitude was determinable, and while its insignificance remained undemonstrated.

He then proceeds to compute the effect of periodical forces with or without resistance, and shows that the effects of such forces on a pendulous or vibratory body are always most considerable when the period of the force approaches very near to that of the vibration: a proposition which is illustrated by the sympathetic vibrations of the pendulums of clocks, and in the motion of the inverted pendulum invented by Mr. Hardy, as a test of the steadiness of a support, which shows, when it is well adjusted to the rate of a clock, that no pillar can be so steady as not to communicate to it a very perceptible motion by its regular, though extremely minute, and otherwise insensible change of place.

The theorem most immediately applicable to the case of the

tides is this, (K*): "the equation, $\frac{dds}{dt^2} + A\frac{ds}{dt} + Bs + M \sin. Gt = 0$, may be satisfied by taking $s = \frac{M}{\sqrt{((GG - B)^2 + AAGG}}$ $\sin. \left(Gt - \text{arc tan}\frac{AG}{B - GG}\right)$." which is also extended by a subsidiary approximation to the case of a resistance varying as the square of the velocity.

In the fourth section† of the article we find the "Astronomical determination of the periodical forces which act on the sea or on a lake," affording the equations which, by means of Theorem K, could give at once the height of the tides in any port, if the coefficients were sufficiently determined, and even without this determination affording some interesting conclusions from facts that are already well known.

For a canal or a sea lying in an easterly and westerly direction, the periodical force is shown to vary as sin *Alt.* cos *Alt.* sin *Az.*, and for a canal deviating from that direction in a given angle, as sin *Alt* cos *Alt.* sin (*Az.* + *Dev.*). And in the two cases of a canal running east and west in any latitude, and of a canal situated obliquely at or near the equator, the force becomes, still more simply, first L sin *Decl.* cos *Decl.* sin *Hor.* ∠ + L′ cos² *Decl.* sin *Hor.* ∠ cos *Hor.* ∠, L being the sine of the latitude, and L′ its cosine; and secondly, if D be the sine of the deviation, or of the angle formed by the length of the canal with the equator, and D′ its cosine, D sin *Decl.* cos *Decl.* cos *Hor.* ∠ + D′ cos² *Decl* sin *Hor.* ∠ cos *Hor.* ∠.

A series is then found for representing the declination by means of arcs increasing uniformly with the time; but it is observed that for the purposes of calculation it is sufficient to suppose the sun and moon to move uniformly in the ecliptic, or even to have uniform motions in right ascension; whence we obtain for the sun's force, on a canal running east and west, putting α for the sine of the obliquity of the ecliptic, ⊙ for the sun's longitude, and t for the horary angle, $S(\text{L}\,\alpha' [\frac{1}{2}\cos(t - \odot) - \frac{1}{2}\cos(t + \odot)] + \text{L}\,\alpha'' [\frac{1}{2}\cos(t - 3\odot) - \frac{1}{2}\cos(t + 3\odot)] + \text{L}\,\alpha''' [\frac{1}{2}\cos(t - 5\odot) - \frac{1}{2}\cos(t + 5\odot)] + \frac{\text{L}'}{2}(-\frac{1}{2}\alpha^2)$ $\sin 2t + \frac{\text{L}'}{4}\alpha^2 [\frac{1}{2}\sin 2(t + \odot) + \frac{1}{2}\sin 2(t - \odot)]$: α', α'',

* Supra, p. 322. † Supra, p. 323.

and a''', being coefficients derived from a, and equal respectively to about .3645, .0078, and .00002, and $a^2 = .1585$. From each of the terms, expressing the forces, the value of the corresponding portion of the space described may be obtained by means of the general Theorem K, substituting, in the case of the solar tide, for the coefficient of the simple resistance A, the value $A' = A + 2.88\ DM'$, in which D is the coefficient of the resistance varying as the square of the velocity, and M' the supposed actual extent of the lunar tide; and for the lunar tide $A'' = A + 2.88\ DS' + .8484\ D(M' - S')$.

But without calculating the precise amount of all the coefficients, the author proceeds to demonstrate in general, that "the results, with regard to the space described, will not differ much from the proportion of the forces, except when their periods approach nearly to that of the spontaneous oscillation, represented by B." And "considering in this simple point of view the correct expression of the force; we may observe that the phenomena, for each luminary, will be arranged in two principal divisions, the most considerable being represented by $\frac{1}{4}$ (L', D') cos² *Decl.* sin 2 *Hor.* ∠, and giving a tide every twelve hours, which varies in magnitude as the square of the cosine of the declination varies, increasing and diminishing twice a year, being also proportional to the cosine of the latitude of the place or of the inclination of the canal to the equator, and disappearing for a sea situated at the pole: the second part is a diurnal tide proportional to the sine of the latitude or of the inclination, being greatest when the luminary is furthest from the equinox, and vanishing when its declination vanishes."

He next proceeds "to inquire more particularly into the cause of the hitherto unintelligible fact, that the maximum of the spring tides in the most exposed situations, is at least half a day, if not a whole day, later than the maximum of the moving forces.

"Now it is easy to perceive that, since the resistance observing the lunar period is more considerable than that which affects the solar tide, the lunar tide will be more retarded or accelerated than the solar; retarded when the oscillation is direct, or when $G' - B$ is [negative,] and accelerated when it is in-

verted, or when that quantity is [positive]; and that, in order to obtain the perfect coincidence of the respective high waters, the moon must be further from the meridian of the place than the sun; so that the greatest direct tides ought to happen a little before the syzygies, and the greatest inverted tides a little after; and from this consideration, as well as from some others, it seems probable that the primitive tides, which affect most of our harbours, are rather inverted than direct."

As a convenient epoch for dating the beginning of a series of tides, it is observed that the mean conjunction, at the beginning of 1824, happens exactly at mean noon of Jan. 1, in the time of the island of Guernsey or of Dorchester, and at 18^m 49^s Parisian mean time.

It is further observed respecting the effects of resistance, that this cause "tends greatly to diminish the variation in the magnitude of the tides, dependent on their near approach to the period of spontaneous oscillation, and the more as the resistance is the more considerable: and supposing, with Laplace, that in the port of Brest, or elsewhere, the comparative magnitude of the tides is altered from the proportion of 5 to 2, which is that of the forces, to the proportion of 3 to 1; the multipliers of the solar and lunar tides being to each other as 5 to 6,... we find that B must be either .9380 or .6328, and the former value making the lunar tide only inverse, we must suppose the latter nearer the truth; and the magnitude of the tides will become 1.663 and 1.998, and $\therefore$ A cannot be greater than .632. It seems probable, however, that the primitive tides must be in a somewhat greater ratio than this of 2 to 1, and 5 to 3, when compared with the oscillations of the spheroid of equilibrium; and if we suppose $B = .9$, and A still $= \frac{1}{10}$, we should have [6.364 and 8.78] for their magnitude;" so that the actual elevations would be about 6 and 19 feet respectively.

"Now ... the tangents of the angular measures of the displacement, $\frac{AG}{GG - B}$, give us 69° 50′ and 72° 40′ for the angles themselves, when $B = .6328$; and if $B = .9$, these angles become 45° and 70° 24′ respectively; the difference in the latter case,

25° 24′, corresponding to a motion of more than 24 hours of the moon in her orbit.

"It appears then that, *for this simple reason only*, if the supposed data were correct, the highest spring tides ought to be A DAY LATER than the conjunction and opposition of the luminaries; so that this consideration obviously requires to be combined with that of the effect of a resistance proportional to the square of the velocity, which has already been shown to afford a more general explanation of the same phenomenon."

It may easily be admitted that this theory may require much further illustration, and perhaps discussion, before it can be rendered very popular, or intelligible in all its bearings; but in point of mathematical evidence, it may not be superfluous to insert here the reduction of the expression of the force acting on an oblique canal into the simple form which the author has adopted, without a demonstration, at the end of his paper.

Since the force $f = \sin \mathit{Alt.} \cos \mathit{Alt.} \sin (\mathit{Az.} + \mathit{Dev.}) = \sin \mathit{Alt.} \cos \mathit{Alt.} (\text{D}' \sin \mathit{Az.} + \text{D} \cos \mathit{Az.})$; and $\sin \mathit{Alt.} = \text{L} \sin \mathit{Decl.} + \text{L}' \cos \mathit{Decl.} \cos \mathit{Hor.} \angle$; also $\sin \mathit{Az.} = \frac{\cos \mathit{Decl.} \sin \mathit{Hor.} \angle}{\cos \mathit{Alt.}}$; we have $\cos \mathit{Alt.} \sin \mathit{Az.} = \cos \mathit{Decl.} \sin \mathit{Hor.} \angle$, and $\cos^2 \mathit{Alt.} \sin^2 \mathit{Az.} = \cos^2 \mathit{Decl.} \sin^2 \mathit{Hor.} \angle = \cos^2 \mathit{Alt.} (1 - \cos^2 \mathit{Az.})$ and $\cos^2 \mathit{Alt.} \cos^2 \mathit{Az.} = \cos^2 \mathit{Alt.} - \cos^2 \mathit{Decl.} \sin^2 \mathit{Hor.} \angle = 1 - \sin^2 \mathit{Alt.} - \cos^2 \mathit{Decl.} \sin^2 \mathit{Hor.} \angle$; whence $\cos \mathit{Alt.} \cos \mathit{Az.} = 1 - \frac{1}{2} (\sin^2 \mathit{Alt.} + \cos^2 \mathit{Decl.} \sin^2 \mathit{Hor.} \angle) + \frac{3}{8} (\sin^2 \mathit{Alt.} + \cos^2 \mathit{Decl.} \sin^2 \mathit{Hor.} \angle)^2 - \frac{5}{16} \ldots$; and finally,
$f = (\text{L} \sin \mathit{Decl.} + \text{L}' \cos \mathit{Decl.} \cos \mathit{Hor.} \angle)(\text{D}' \cos \mathit{Decl.} \sin \mathit{Hor.} \angle + \text{D} [1 - \frac{1}{2} (\sin^2 \mathit{Alt.} + \cos^2 \mathit{Decl.} \sin^2 \mathit{Hor.} \angle) + \frac{3}{8} \ldots])$; which may readily be more completely developed if required.

But for a lake obliquely situated at the equator, when $\text{L} = 0$, and $\text{L}' = 1$, the expression becomes $\sin \mathit{Alt.} = \cos \mathit{Decl.} \cos \mathit{Hor.} \angle$, and $\cos^2 \mathit{Alt.} \cos^2 \mathit{Az.} = 1 - \cos^2 \mathit{Decl.} \cos^2 \mathit{Hor.} \angle - \cos^2 \mathit{Decl.} \sin^2 \mathit{Hor.} \angle = 1 - \cos^2 \mathit{Decl.} = \sin \mathit{Decl.}$, and $\cos \mathit{Alt.} \cos \mathit{Az.} = \sin \mathit{Decl.}$; whence
$f = \cos \mathit{Decl.} \cos \mathit{Hor.} \angle (\text{D}' \cos \mathit{Decl.} \sin \mathit{Hor.} \angle + \text{D} \sin \mathit{Decl.})$ $= \text{D}' \cos^2 \mathit{Decl} \sin \cos \mathit{Hor.} \angle + \text{D} \sin \cos \mathit{Decl.} \cos \mathit{Hor.} \angle$, which is the equation given at the end of the Article, agreeing with

the equation of the form for a canal running east and west, in having for each luminary a semidiurnal tide which is greatest when the declination vanishes, and a diurnal tide increasing, on the contrary, as the sine of twice the declination increases. The two formulæ give the same result for a canal coinciding with a part of the equator, and they appear in other cases to represent the force for every part of the same oblique great circle, the deviation at the equator being equal to the latitude when it becomes perpendicular to the meridian.

Laplace, assisted by the indefatigable Bouvard, has lately published a very valuable continuation of his Researches on the Tides, as a XIIIth Book of his *Mécanique Céleste*, Febr. 1824. He has computed the results of about 6000 observations made at Brest since the year 1806, and has found them to confirm in general those which he had obtained from the more ancient observations. There are also some new deductions, which may be made subservient to the further illustration of the principles laid down in the Supplement of the Encyclopædia.

"I have considered," says Mr. Laplace, (P. 160,) "the tide of which the period is about a day. By comparing the differences of two high and two low waters, following each other in a great number of solstitial syzygies, I have determined the magnitude of this tide, and the time of its maximum, for the port of Brest. I have found its height very nearly one-fifth of a metre, and one-tenth of a day for the time that it precedes the time of the maximum of the semidiurnal tide. Though its magnitude is not one thirtieth of that of the semidiurnal tide, yet the generating forces of both these tides are nearly equal, which shows how differently their magnitude is affected by accidental or extraneous circumstances. This will appear the less surprising, when we consider that if the surface of the earth were regular and entirely covered by the sea, *the diurnal tide would disappear, provided that the depths were uniform throughout*." In fact, the observed heights of the diurnal and semidiurnal tides are $.2134^{m}$, and 5.6^{m} respectively, (P. 227;) and the time that the diurnal tide precedes the maximum of the evening semidiurnal tide is $.095^{d}$ (P. 226). It is not quite clear that the words might not relate to the maximum result-

ing from the most perfect combination of the solar and lunar diurnal tides; but we may suppose, for the sake of the calculation, that the high water of the joint diurnal tide generally happens a little more than two hours earlier than that of the semidiurnal tide.

Now supposing, for the determination of the multiplier, $\frac{B}{\sqrt{[(GG-B)^2+AAGG]}}$ we assume the mean value of G, for the joint semidiurnal tide, about .98, and for the diurnal .49, B being about .9, and $A = .1$, the formula becomes $= 7.83$, or if $A = .2$, 4.4 for the semidiurnal, and 1.327 or 1.234 respectively for the diurnal, and $\frac{\text{D}}{\text{D}'}$ or $\frac{\text{L}}{\text{L}'}$ must be such that D sin 2 *Decl.* $\times$ 1.327 may be to D$'$ $\times$ 7.83 as .2134 to 5.6, or as 1 to 26.25; but sin 2 *Decl.* = sin 46° 55′.5 = .73045, and we have D $\times$ .9691 : D$'$ $\times$ 7.83 = 1 : 26 25 = D : D$'$ $\times$ 8.07 and D : D$'$ $= 1 : \frac{26.25}{8.07} = 3.25 =$ cot 17° 6′, which must be the obliquity of the canal to the equator if $A = .1$, or if $A = .2$, 10° 30′: either of which may possibly be near the truth, though the obliquity of the main channel of the Atlantic to the equator is probably greater. With respect to the times of high water, the tangents $\frac{\beta}{\alpha} = \frac{AG}{GG-B}$ become, if $A = .2$, at 72° 59′ and at 8° 27′ respectively; the former expressing the acceleration of the inverted semidiurnal tide, and the latter the retardation of the direct diurnal tide, by the effect of friction, the sum of the former and twice the latter is 89° 53′, or very nearly a right angle; so that the interval, thus computed, instead of one tenth of a day should be a little more than an eighth. It would, however, be necessary to compare the height of the water at different intervals before and after high water, in order to obtain the progressive magnitude of the diurnal tide with sufficient accuracy to allow us to place any reliance on the result of this computation.

With respect to the disappearance of the diurnal tide in an ocean of equable depth, no doubt the depth *must* be *equable* in order that it may disappear, but it must *also* be *evanescent*. In fact, it is not conceivable in what other manner the equability

of depth can possibly produce such an effect; for there is no natural nor assignable relation between the period of revolution and that of diurnal tide; the effects are just the same as if the earth were at rest, and the attracting body moved round in a day, or in two days; and it is impossible to admit the accuracy of any refined method of investigation, from which Mr. Laplace has obtained a result so clearly contradictory to the first principles of mechanics.

No. LVII.

AN ALGEBRAICAL EXPRESSION FOR THE

VALUE OF LIVES,

WITH THE MODE OF FINDING THE CORRECT VALUE OF ANY NUMBER OF JOINT LIVES.

From the Philosophical Magazine for 1816, vol. xlvii. p. 1.

IN making calculations from the registers of the duration of lives, it has not been usual to attempt to represent the results of a whole table by a single formula, although such a simplification would afford great advantages in the solution of a variety of problems. As an instance of this mode of treating the subject, we may take the bills of mortality for the year now elapsed, and by reducing the numbers into the form of a diagram, and observing the different flexures of the curve resulting from them, we may ascertain the nature of the terms proper to constitute the required expression; and calling the age x, the annual deaths will appear to be $\frac{1}{4} \cdot \frac{1}{1+xx} + {\cdot}000401x - {\cdot}0000042x^2$, without any very material error:* at least with incomparably greater accuracy than is obtained by the rough approximation of supposing an equal annual mortality in a given number of persons as long as any of them remains alive; and much more nearly than the registers of a metropolis agree with those which have been kept in the country. It would be

* It will be found that nearly all Dr. Young's investigations on the value of lives and annuities are based upon *empirical* formulæ, constructed to represent the results of observations embodied in tables and calculations, and adapted to correct the irregularities of such tables, which defective observations may have occasioned; a process in some respects equivalent to drawing a curve line through or near a series of points, so as to avoid abrupt changes and to preserve the continuity which usually characterises the operation of natural causes. A formula of a much more complicated and, we may add likewise, of a much less legitimate description for expression the decrement of human life, is given in the article which follows, No. LVIII.—*Note by the Editor.*

very easy to correct the expression from the average of the registers of a number of years; and the formula might also be adapted with little difficulty to the mortality observed in any other situation.

In order to find the number of persons who have died during any given portion of the period of the longest human life, we must combine the expression for the mortality with the fluxion $\dot{x}$ of the age; the fluent, $\frac{1}{4}$ arc tang. $x + \cdot 0002005x^2 - \cdot 0000014x^3$, will indicate the aggregate amount of deaths at the given age. It has sometimes been thought sufficiently accurate to estimate the value of a life by the period at which half the persons who have attained the given age are likely to remain alive: this affords a true criterion of the greatest probability of the duration of one life, but not of the correct mean value of a number of lives; since this value can only be found by dividing the aggregate duration of all the lives in question by their number: now this aggregate duration is represented by the area of the curve having the ages and the numbers living, for its ordinates and abscisses; and this area is obtained from the fluent $x - \frac{1}{4}x$ arc tang. $x + \frac{1}{8}$ HL $(1 + x^2) - \cdot 00006683x^3 + \cdot 00000035x^4$. Hence we may calculate the mean value of life in London, as shown in the following table. At birth, the most probable duration is about 27 years; the mean value more than 30.

AGE.	DEATHS. Registered.	DEATHS. Calculated.	MEAN VALUE.	HALLEY. (BRESLAU.)	$43-\frac{1}{2}x$.
0	000	·0000	30·29		43
1		·1954	36·53	33·5	42½
5	385	·3480	40·80	41·2	40½
10	409	·3864	38·21	40·4	38
20	444	·4492	31·80	34·2	33
30	517	·5269	26·60	27·9	28
40	610	·6177	21·46	22·3	23
50	717	·7140	16·96	17·2	18
60	813	·8080	12·90	12·4	13
70	896	·8914	9·09	7·6	8
80	959	·9559	5·17	4·5	3
90	994	·9933	1·64		
95		·9993			
100	999·9				

The advantage of this method of calculation is strongly exemplified in the determination of the value of the joint continuance of two or more lives, which may be obtained by means of the quadrature of a curve having its ordinates in the joint ratio of the survivors at the time expressed by the absciss; the corrected area being divided by the product of the numbers of survivors at its commencement, which obviously expresses the number of all the possible combinations of their lives, as the area does the aggregate duration of those combinations.

For this purpose it will be convenient to represent the number of deaths by the formula $\cdot 38 + \cdot 0002x^2 - \cdot 00000137x^3$, which will be sufficiently accurate for any age exceeding 10 years; and if we make $a = \cdot 62$, $b = \cdot 0002$, and $c = \cdot 00000137$, and call the two ages x and $x + n$, the product of the numbers of survivors will be $d + ex + fx^2 + gx^3 + hx^4 + ix^5 + kx^6$, where $d = a^2 - abn^2 + acn^3$, $e = 3acn^2 - 2abn$, $f = b^2n^2 - 2ab + 3acn - bcn^3$, $g = 2b^2n - 4\,bcn^2 + 2dc + c^2n^3$, $h = b^2 - 3bcn + 3c^2n^2$, $i = 3c^2n - 2bc$, and $k = c^2$; and the area of the curve will be $dx + \frac{1}{2}ex^2 + \frac{1}{3}fx^3 + \frac{1}{4}gx^4 + \frac{1}{5}hx^5 + \frac{1}{6}ix^6 + \frac{1}{7}kx^7$, which must be found for the given age, and for $x + n = 98$ or 100, and the difference, divided by the product, will show the value of the joint lives.

But in pursuing the calculation for a greater number of lives, it would be necessary to assume a still simpler expression for the number of deaths, such as mx, m being $\frac{1}{86}$, or from $\frac{1}{80}$ to $\frac{1}{90}$ according to circumstances, retaining the more accurate expression for the elder lives; and taking for the age of the younger $x - p$, and for the number of the survivors $1 - mx + mp$, which may be called $q - mx$: and the former product might be multiplied by this for the case of three lives, and the area found as before. Indeed this expression may be employed for the younger of two lives without material inaccuracy, the product becoming $aq - amx - bqx^2 + (cq + bm)\,x^3 - cmx^4$, and the area $aqx - \frac{1}{2}amx^2 - \frac{1}{3}bqx^3 + \frac{1}{4}(cq + bm)\,x^4 - \frac{1}{5}cmx^5$; whence, for example, when the ages are 10 and 20, supposing $m = \cdot 012$, we obtain the mean joint value $22\cdot 9$: nor would the

result in this case be materially different if we employed the same simple estimate of the mortality with respect to both lives, though it would vary more at other ages. We may, however, safely make the value of m = ·012 between the ages of 20 and 60 in London, even for the case of three joint lives, the number of survivors being called $1-mx$, $1+mp-mx$, or $q-mx$, and $1+mr-mx$, or $s-mx$, respectively; p and r being the differences between the eldest and the two younger lives: the product of these will be $qs - m(s+q+qs)x + m^2(1+q+s)x^2 - m^3x^3$, and the area $qsx - \frac{1}{2}m(s+q+qs)x^2 + \frac{1}{3}m^2(1+q+s)x^3 - \frac{1}{4}m^3x^4$, which must be found for $mx = 1$, and for the given age, and the difference divided by the product.

When two lives are equal, the mean value of their joint continuance, thus approximated, becomes exactly two-thirds of that of a single life; of three, two-fourths or a half; of four, two-fifths; of five, two-sixths, and so forth: whence also we obtain for the value of the longest of two lives, $\frac{4}{3}$ of that of a single life, and for the longest of three $\frac{6}{4}$; and we may continue the series at pleasure by adding at each step 2 to the numerator and 1 to the denominator.

According to the usual method of estimating the value of three joint lives from that of two lives, one of which is of equal value with two of the three taken together, the result, in the case of equal lives, is about $\frac{14}{27}$ of the value of a single life, instead of half; that is, almost 4 per cent. too much, an error by no means to be neglected in practice. It will be easy to obtain a more correct approximation, from the principles here explained, employing any tables of the value of lives that may be preferred. Let m be found for the eldest life, by making $\frac{1}{m}$ equal to twice its value, increased by the age: and let t and u be found in the same manner for the other two lives, so that the numbers of survivors may be denoted by $1 - mx$, $q - tx$, and $s - ux$, q being $1 + tn$, and $s = 1 + ur$: the area will then be $(qs - \frac{1}{2}(qsm + qu + st)x + \frac{1}{3}(tu + omu + smt)x^2 -$

$\frac{1}{4} mtux^3)\ x$, which must be taken for the given age of the eldest life and for $mx = 1$, and the difference divided by the product of the survivors will give the value of the three joint lives, with much greater accuracy than it can be determined in the manner directed in the Legacy Duty Act.

This remark is, however, only strictly correct, with regard to the precise amount of the error in question, when the age is so great, that the different effects of the operation of interest on the relative pecuniary values of the lives may be disregarded. It is obvious that the preceding calculations are wholly independent of this consideration, giving us only a theoretical mean value, from which it is not possible to deduce immediately a practical value, without further reference to the comparative numbers of the survivors at different ages, in order to estimate the various operation of interest, and particularly of compound interest. But the same mode of computation may readily be extended, so as to comprehend the effect of interest also, by supposing the ordinates, expressing the number of survivors, to be reduced in the proportion of the present value of a given sum payable at the corresponding time: that is, by multiplying the expressions denoting them by v^x; v being the present value of a unit payable at the end of a year.

But the expression containing the circular arc becomes intractable, even when applied to the termination of the present value of a single life: we may therefore take, instead of the primary expression $\frac{1}{1+xx}$, some members of the equivalent series $\frac{1}{x^2} - \frac{1}{x^4} + \frac{1}{x^6} \ldots$, and make the fluxion $\frac{1}{4}\dot{x}\left(\frac{1}{x^2} - \frac{1}{x^4}\right)$, of which the fluent is $C - \frac{1}{4x} + \frac{1}{12x^3}$; and this, if we put $C = \cdot405$, will be sufficiently accurate even in infancy. We shall then have, for the number of survivors at the age x, $\cdot595 + \frac{1}{4x} - \frac{1}{12x^3} - \cdot0002005x^2 - \cdot0000014x^3$; which multiplied by $v^x\dot{x}$, will give the fluxion of the area.

The fluent may be found by means of the following theorems of Euler's *Calculus Integralis* (§ 228, 225, 224); $\int\frac{v^x\dot{x}}{x} =$

$$\text{HL}x + x\text{HL}v + \frac{x^2(\text{HL}v)^2}{1.2.2} + \frac{x^3(\text{HL}v)^3}{1.2.3.3} + \frac{x^4(\text{HL}v)^4}{1.2.3.4.4} \ldots (= \text{X});$$

$$\int\frac{v^x \dot{x}}{x^3} = \frac{(\text{HL}v)^2}{2}\,\text{X} - \frac{v^x}{2x^2} - \frac{v^x\,\text{HL}v}{2x}; \quad \int v^x\,\dot{x} = \frac{1}{\text{HL}v}\,v^x;$$

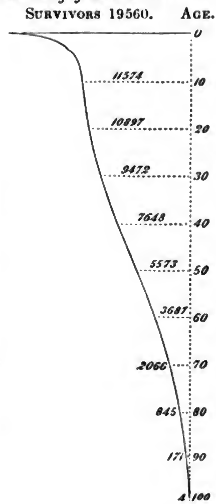
$$\int v^x\,x^2\,\dot{x} = v^x\left(\frac{x^2}{\text{HL}v} - \frac{2x}{(\text{HL}v)^2} + \frac{2}{(\text{HL}v)^3}\right); \text{ and } \int v^x\,x^3\,\dot{x} =$$

$v^x\left(\frac{x^3}{\text{HL}v} - \frac{3x^2}{(\text{HL}v)^2} + \frac{3.2x}{(\text{HL}v)^3} - \frac{3.2}{(\text{HL}v)^4}\right)$. When x is large, the series converges somewhat slowly: but its utility is chiefly confined to the earlier ages; and the ultimate value, being once only computed, for $x = 100$, will serve for the correction in all other cases: but after the age of 10, the formula $a - bx^2 + cx^3$, already mentioned, will be more convenient. The fluent, when corrected, by subtracting it from its ultimate value, must be divided by the number of survivors, and multiplied by $\frac{1}{v^x}$, since it would otherwise express the value of the continued payment at the time of birth, instead of the actual present value.

The same solution may be applied to the expressions for the value of joint lives, multiplying always the product of the survivors by $v^x\,\dot{x}$; the fluents being all comprehended in the general theorem $\int v^x\,x^n\,\dot{x} = v^x\left(\frac{1}{\text{HL}v}\,x^n - \frac{n}{(\text{HL}v)^2}\,x^{n-1} + \frac{n(n-1)}{(\text{HL}v)^3}\,x^{n-2} - \frac{n(n-1)(n-2)}{(\text{HL}v)^4}\,x^{n-3} + \ldots\right)$. For the joint present value of three or more lives, the quantities m, t, and u, may be determined from the theoretical mean values of each, or otherwise, and the present value, thus approximated, will differ much less from the truth than the theoretical mean values would do, if originally found in a similar manner. The usual deduction must be made for periodical payments, whether annual or quarterly: and a slight correction may sometimes be required, on account of the different effects of annual and continual interest; but both these points are very easily arranged: commonly, indeed, the subtraction of half a payment from the present value, thus computed, will give us the corrected value with considerable accuracy.

The great labour required for such calculations, according to the usual methods, renders it very difficult to adapt the tables of annuities to every possible variation of the value of life; the improved habits of society, and probably also the advancement of the medical sciences, and especially the introduction of vaccination, seem to have effected by degrees a very material change in the mortality of this metropolis; and it appears that the magnitude of such a change, and its operation, in its various modifications, may in many cases be much more conveniently appreciated by this mode of finding a continuous law for the decrements of life, than by the inspection of tables, and the numberless combinations of their elements.

Mortality of London 1815.



No. LVIII.

A FORMULA FOR EXPRESSING THE DECREMENT OF

HUMAN LIFE.

IN A LETTER ADDRESSED TO SIR EDWARD HYDE EAST, BART., M.P.

From the Philosophical Transactions for 1826.

READ APRIL 19, 1826.

MY DEAR SIR,

THE investigation of the laws by which the general mortality of the human species appears to be governed, is of equal importance to the statesman, the physician, the natural philosopher, and the mathematician; and as you have had occasion to pay particular attention to the subject, I trust that it will not be disagreeable to you to receive the results of an inquiry, into which I have entered, for the purpose of appreciating, if not of reconciling, the many discordant opinions that have been advanced, respecting the comparative mortality of mankind, at different times, and under different circumstances.

Of late years there is little doubt that, whether from the protective effects of vaccination in infancy, or from the increase of the comforts of the poorer, and of the temperance of the more affluent classes of society, or in some measure also from the simplification of the practice of physic and surgery, there is a decided increase in the mean duration of life in many parts of Europe: but it is also extremely probable that this improvement has been greatly exaggerated; partly on account of the limited description of the persons on whom the observations have been made, and partly from an erroneous opinion respecting the profits of certain establishments, which have been attributed to the employment of too low an estimate of mortality, while they have, in fact, been principally derived

from the high rate of interest which the state of public credit has afforded.

A very laborious and well informed actuary has lately asserted, before a Committee of the House of Commons, that "the duration of existence now, compared with what it was a hundred years ago, is as four to three in round numbers." (Parl. Rep., N. 522, p. 44.)* It does indeed happen that this particular result may in one sense be very correctly deduced from the immediate comparison of the annual mortality of a certain number of persons of the same description, that is, annuitants, at the periods in question; nor is it possible to deny that some importance must be attached to the remark: but the mortality of the same class of persons in France, at the earlier period, was no greater, according to M. Deparcieux's estimate of their longevity, than in England at the later, while the general mortality in France has never been materially less than in England, and appears at present to be even somewhat greater: and it can only be conjectured, that the annuitants of the tontine of King William were in general most injudiciously selected, while those who were the subjects of M. Deparcieux's observations, like the annuitants of the modern tontines, were chosen with more care, or with greater success. Mr. Finlaison's tables, therefore, though they may be extremely just and valuable for the purpose of setting a price upon annuities to be granted on the lives of the proposers, cannot, with any prudence, be adopted where the parties concerned have an interest in offering the worst lives that they can find; notwithstanding any partial security that might be afforded by the exercise of medical skill in their rejection: and if it is true, that some of the tontines were principally filled by lot (Rep. p. 16), with the children of country clergymen and magistrates, it must still be supposed that the families of such persons may have been more healthy than the average of the population of London and the country taken together.

For the comparison of the general characters of different tables of mortality, the simplest and most obvious criterion is perhaps the number of individuals out of which one dies

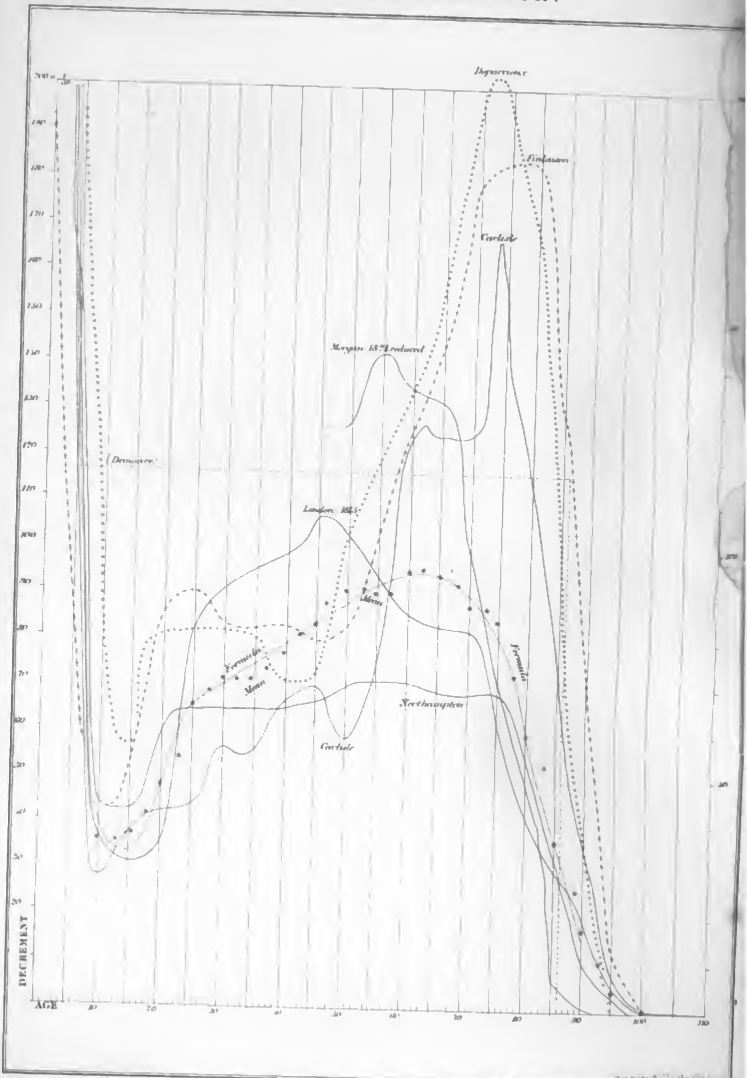
* Mr. Finlaison, of the National Debt Office.—*Note by the Editor.*

annually, which is also the number of years expressing the expectation of life at the time of birth. But this test is liable to material objections with regard to the most usual application of the table, which depends more on the comparative expectations at later periods than in early infancy. For example; the Northampton table affords results, throughout the whole of middle and advanced life, agreeing almost exactly with Demoivre's hypothesis of equal decrements, although the *annual mortality* is supposed to be nearly 1 in 25 at Northampton, instead of 1 in 43, as assumed by Demoivre. It would therefore be very unjust for a person allowing the truth of Demoivre's hypothesis, to condemn the practical employment of Dr. Price's tables in common cases, on account of this variation only. A less exceptionable test will be, to find the mean of the numbers expressing, for different ages, the *full term* of life, or the sum of the age and twice the expectation, taking the decads from 10 to 80 as the most important. Another standard of comparison may be the age which is equal to the expectation of life, and which, in Demoivre's arithmetical hypothesis, is the *mean age* of all the population, and probably very near it in all tables formed from actual observation. In this manner a general comparison of the most remarkable tables may be instituted.

Characteristics of Mortality.

—	Annual mortality one in	Mean full term of life.	Mean age.
Roman estimate of Ulpian, probably with some deduction for present value	.....	74 (+disc^t)	26 (+disc^t)
Deparcieux's Tontines, beginning 1689	47.67	94.17	32.5
Halley's table for Breslau, 1690	33.50	87.15	28.1
Tontine of 1695, Finlaison, males	37.61	83.42	27.25
———, females	(43.0)	87.50	29.32
Simpson's table for London, about 1730	19.2	82.30	25.7
Dupré, in Buffon, about 1750	33.0	85.30	28.67
Northampton tables, about 1760	25.18	87.39	28.86
Swedish tables, about 1785	36.12	91.86	31.3
France, before the Revolution, Duvillard	28.76	86.96	29.0
Finlaison's tontine and annuitants, about 1800,			
males	50.16	93.25	32.0
females	55.51	100.7	34.6
Finlaison's Chelsea Pensioners	.....	90.0	29.65
Carlisle tables, about 1810, Milne	37.14	95.47	32.6
Returns for all England, 1811	49		

DECREMENT OF HUMAN LIFE.



To face page 369. Vol. II.

The order of the mortalities expressed by the first column of this table is Simpson, Northampton, France, Dupré, Halley, Sweden, Carlisle, tontine 1695 males, females, Deparcieux, returns of 1811, tontines of 1800, males, and females; the order of the second column is Simpson, tontine of 1695, males, Dupré, France, Halley, Northampton, females 1695, pensioners, Sweden, tontines of 1800, males, Carlisle, females of 1800; but besides this difference in the order, the disproportion exhibited in this column is less enormous than in the former; the numbers of the Carlisle tables, for example, exceeding those of the Northampton by one half in the former, and by one tenth only in the latter. The proportion of Mr. Finlaison's tontines also stands as 3 to 4 in the first, and as 7 to 8, or 8 to 9 only in the second; the latter comparison giving a much fairer practical estimate of the comparative longevity indicated by the tables than the former.

Another mode of easily appreciating the regularity and the analogies of different tables is, to construct a diagram, in the form of a curve, of which the absciss represents the age, and the ordinates the corresponding decrements of life. (See Plate.) The inspection of such a diagram is sufficient to convince us of the great irregularity of the Carlisle tables of mortality, which must obviously have been formed, as they confessedly were, from observations on a very limited number of individuals, so that they exhibit a succession of different climacterics, after which the mortality is diminished, while about the age of 74 the curve that represents them towers to an incredible height, affording an expectation of longevity which some of the strongest advocates of those tables have abandoned in their practical applications, since they take their estimate of life, in advanced age, even lower than it is represented in the Northampton tables.

It appears therefore to be highly probable, that the fairest basis for general computations, to be applied throughout Great Britain, may be obtained by a proper combination of the tables of Northampton, which have been long known and very generally approved, with the Carlisle tables, corrected however in their extravagant values of old lives, by some other documents;

and with the mortality of London as derived from the parish registers, which, when thus incorporated with tables formed in the country, will be freed from the objections that have been made to the observations of burials in great cities only.

The Carlisle table agrees in the earlier parts pretty nearly with the observations of Mr. Morgan on the experience of the Equitable Office from 1768 to 1810, as it appears from Mr. Milne's comparison, as well as from the reduction and interpolation of those observations published by Mr. Gompertz in the Philosophical Transactions for 1825; but for correcting the later portions of the Carlisle table, it may be allowable to employ a subsequent register of the experience of the Equitable Office, so far as it is possible to make any inferences from it with safety.

The numbers of deaths occurring in 20 years, as recorded by Mr. Morgan, might have been made the foundation of a very valuable determination of the mortality occurring in a certain class of persons, if the number of the Equitable Society had become stationary before the commencement of the record: but in order to deduce from it a just estimate of the value of life, it would then be necessary to alter the numbers of deaths at each age, in the inverse proportion of the numbers of the living compared: that is to say, not simply of the sums of the persons admitted under that age, but of the numbers of persons born whom they represent: since, in comparing the joint mortalities of any two lists of persons, we must obviously add together the deaths belonging, not to a given number of persons of various ages, but of a number proportionate to the survivors at the respective ages out of a given number of births, so that in this manner the apparent mortality in the earlier portions of the register would require to be augmented, not only on account of the smaller number of persons who have actually contributed to furnish it, but also on account of the greater proportion that these persons bear to the corresponding number at birth, when compared with the survivors at more advanced ages, who represent a population still more exceeding their own numbers On the other hand, since the register in question relates only to a limited number of years, immediately

following a very rapid increase of the Society, it is evident that the deaths must have occurred at earlier ages than if it had been continued until all the lives had dropped.

Of these three modifications, it may be sufficiently accurate for the present purpose to omit the two latter as nearly counterbalancing each other, and to augment the earlier numbers in the proportion only of the members of the Society to whom they must necessarily have belonged, supposing that the admissions had taken place about the same ages at all periods; assuming also the number of survivors at 45 to be in the same proportion to the births as in the Carlisle table. We may then proceed to take a mean between the mortality thus obtained, with proper interpolations, and the observations at Carlisle, as the second of the three principal bases to be afterwards incorporated with the mortality of Northampton and of London. Further than this it is impossible to place any great reliance on Mr. Morgan's document, which makes the annual deaths, in "a population exceeding 150,000," not quite 1 in 1500.*

Of the mortality of London, taken for the ten years from 1811 to 1820, it may be observed, that its results bear the internal evidence of greater apparent correctness than either of the other bases, exhibiting a curve less irregular in its flexures, and generally intermediate between the others: it has also the advantage of exhibiting the duration of life as prolonged by the general introduction of vaccination, and when thus incorporated with the registers of two places in the country, each reduced to an equal supposed population, it must probably be sufficiently corrected for the errors that may be attributed to the effect of an afflux of settlers at an early age. The mean obtained in this manner might be employed at once as a standard table without much inconvenience, but it still exhibits some minute but obvious irregularities, as an inspection of the line of stars in the diagram will show, principally

* This is obviously a mistake. Mr. Morgan, in the 'Annals of Philosophy' for January, 1828, says that "in no part of his works had he ever made such an assertion." In the Nosological Table at the end of his 'Treatise on Annuities,' he represents the number of deaths in each year to be as 1930 to 151,734, or as 1 to 78 nearly.—*Note by the Editor.*

perhaps from the want of skill or care with which the interpolations have been made by Dr. Price and others. The most effectual of all interpolations for *harmonizing* the various orders of differences, is to obtain a formula which shall extend with sufficient accuracy throughout the whole curve. It may be easily believed that it must be extremely difficult to find such an expression; and that its form must be too complicated to be applied to any practical purpose throughout its extent. I have however drawn a curve which comes extremely near to the line of stars, and crosses it in 10 or 12 different points, by means of the equation $y = 368 + 10\,x - .11\,(156 + 20\,x - xx)^{\frac{3}{2}} + \frac{1}{285+2.05xx+2\left(\frac{x}{10}\right)^{6}} - 5\,.\,5\left(\frac{x}{50}\right)^{10} + \frac{5\,.\,5^{2}}{4000}\left(\frac{x}{50}\right)^{20} - 5500\left(\frac{x}{100}\right)^{40}$: y being the number of deaths among 100,000 persons in the year that completes the age x.*

The terms of this formula have some remarkable relations to the different periods of life. Halley's first approximation was $y = 1000$, throughout life. Demoivre's arithmetical hypothesis was $y = \frac{100000}{86} = 1163$: but of the present formula the principal foundation, as extending to the whole of life, is $y = 368 + 10\,x$. In infancy, the term containing the reciprocal of the powers of x has a preponderating value: in youth, the term $-(156+20x-xx)^{\frac{3}{2}}$, which diminishes the mortality, ends somewhat abruptly at 25, and would be incapable of being employed with safety in algebraical calculations, from its having a negative as well as a positive value. Old age is expressed almost exclusively by the high powers at the end of the formula, which terminate the series with great and increasing rapidity. It is obvious that for many purposes of calculation, the terms belonging to youth and to old age might be neglected without inconvenience, and that for the middle

* In the original, the fourth term is printed $\frac{1}{285 + 2.05\,xx + 2\left(\frac{x}{10}\right)^{6}}$ instead of $\frac{100000}{2.85+2.05xx+2.\left(\frac{x}{10}\right)^{6}}$.

Note by the Editor.

portion of life, the terms $368 + 10x$ alone, with some little modification, might be employed as sufficiently correct; or certainly as much nearer to the truth than either the arithmetical or geometrical hypothesis of Demoivre.*

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I shall take this opportunity of endeavouring to demonstrate, in a simple and undeniable manner, the error into which Dr. Price and his followers have fallen, in consequence, as it appears, of their adopting the legal restraints on usury as essential steps in the mathematical calculation of the amount of compound interest. The error has indeed of late years been very commonly admitted; but its effects have not been so satisfactorily rectified as could be desired.

In the 66th volume of the Philosophical Transactions, for the year 1776, we find a paper of Dr. Price, in which he lays down these theorems, r denoting the interest of £1 for a year, and n the term or number of years during which any annuity will be paid, p the perpetuity, or $\frac{1}{r}$, y the value of an annuity paid yearly, and h half-yearly: then, I, $y = p - \frac{1}{r(1+r)^n}$; and II, $h = p - \frac{1}{r\left(1+\frac{r}{2}\right)^{2n}}$: and as examples, taking $r = .04$ and $n = 5$, we have $y = 4.4518$, and $p = 4.4913$.

Now, if we analyse the results thus obtained, by dividing them into the present values of the separate payments, they will stand thus:

* In the original Memoir a series of tables are given for the purpose of showing the relations of the different parts of the preceding formula, as well as of the formula itself, in various tables of mortality, to different periods of life. It has not been thought necessary to reprint them.—*Note by the Editor.*

I. Present value of £1. payable at the end of 1 year	-	£. .961538
2 years	- -	.924556
3 years	- -	.888996
4 years	- -	.854804
5 years	- -	.821927
		4.451821

II. Present value of 10 shillings, payable at the end of half a year	-	£. .49020
1 year	- -	.48058
$1\frac{1}{2}$	- - - -	.47127
2	- - - - -	.46192
$2\frac{1}{2}$	- - - -	.45286
3	- - - - -	.44398
$3\frac{1}{2}$	- - - -	.43528
4	- - - - -	.42674
$4\frac{1}{2}$	- - - -	.41837
5	- - - - -	.41018
		4.49138

The present values of 10 shillings are therefore assumed;

I, at 1 year	.48077;	II, .48058
2 years	.46228	.46192
3 years	.44450	.44398
4 years	.42740	.42674
5 years	.41096	.41018

The latter column exhibiting obviously a larger deduction for discount than the former; so that the rate of interest in the two calculations is by no means the same: although in the case of $r = .05$, they would respectively represent the highest rate of interest allowed by our laws to be received without a new investment or engagement: but this arbitrary restraint ought certainly not to affect the mathematical consideration of the question.

The difficulty, if any person thinks it such, may be avoided by a mode of investigation which I have lately had occasion to point out. "An annuity, of which a payment is due on a given day, is more valuable than an annuity purchased on that day, and to commence a year after, by the amount of a year's payment: and *the value of a life annuity, becoming payable at any intermediate time between the day of purchase and its first anniversary, will be greater than the simple tabular value of the annuity by a sum proportional to the anticipation of the payment;* the increase of the value being very nearly uniform, when we suppose the anticipation to be gradually increased; this increase of the value comprehending obviously the greater probability as well as the greater proximity of each payment, and proceeding from day to day by very nearly equal increments. Thus if we wished to purchase an annuity of 100*l.* a year, and its value were 1000*l.*, upon the ordinary supposition of the payments commencing after the end of a year; supposing that we desired to have the first payment made at the end of nine months, and the subsequent payments at annual intervals as usual, we should have to add 25*l.* to the purchase money, making it 1025*l.* at whatever rate of interest the value might have been computed. If we began at six months, 50*l.*, and if at three months, 75*l.* must be added to the purchase; it being obvious that an additional 100*l.* would be equivalent to an anticipation of twelve months, or to an immediate payment of a year's annuity.

From this simple and incontestable principle, in which the second differences only are neglected, it is very easy to deduce the values of annuities, payable at intervals shorter than a year An annuity of 1, payable half-yearly, is equal to two annuities of $\frac{1}{2}$, the one beginning as usual at the end of the year, the other anticipated by half a year; and the value of this portion is greater than the other by half of one of the payments, that is, by $\frac{1}{4}$: so that "*We may always find the value of a life annuity payable half-yearly by adding a quarter of a year to the tabular value of the same annuity.*"

In a similar manner it is very easily shown that "*for*

quarterly payments, we must add $\frac{3}{8}$ of a year's value to the computation made on the supposition of annual payments;" and "the continual bisection of the interval would at last afford us *the addition of half a yearly payment for the value of a daily or hourly payment of a proportional part of the given annuity.*"

"It may also be observed, that when we reckon at 3 per cent. interest, an annuity payable half-yearly is the same throughout the middle of life, that would be granted on the life of a person a year older, if payable annually."

If it is required to ascertain the value of a reversionary annuity payable half-yearly or quarterly, the calculation becomes in appearance a little paradoxical; for since the true value of a reversionary annuity for the life of one person, for example, after the death of another, is the difference between the values of two annuities on the single life and the joint lives, and since an equal addition must be made to these values in consideration of the period of payment being shortened, it follows that the reversionary annuity must be of equal value in either form. This conclusion would indeed be strictly true if the periodical times of payment remained unaltered, according to the supposition from which the value of the annuities is deduced; while in fact it is usual to grant such an annuity to commence at the first quarterly, half-yearly, or annual period after the contingent event: a variation which would have no sensible effect in the case of daily payments, but which lessens the value of reversionary annuities at other periods by that of half a payment for the given period, reduced to the present time in the same manner as any other sum assured as payable upon the same contingency of survivorship.

The simplicity observable in the progression of the values of annuities, calculated according to the values of lives here supposed, and at 3 per cent. interest, leads us to inquire what would be the exact law of mortality required to make that progression strictly uniform throughout life; and it will appear on investigation, that in order to have the value $24.45-\frac{1}{4}x$, x being the age of the person, which is nearly true between 20

and 70, the annual mortality must be expressed by $\frac{.03\,x + .066}{100.8 - x}$: a fraction which at 20 becomes $\frac{1}{121}$, at 40, $\frac{1}{48}$, at 60, $\frac{1}{22}$, and at 80, $\frac{1}{8.4}$. Our table gives respectively $\frac{1}{103}$, $\frac{1}{50}$; $\frac{1}{23}$, and $\frac{1}{7.3}$: the Northampton $\frac{1}{71}$, $\frac{1}{48}$, $\frac{1}{25}$, and $\frac{1}{7.4}$. Mr. Finlaison's male annuitants $\frac{1}{87}$, $\frac{1}{73}$, $\frac{1}{32}$, and $\frac{1}{8.3}$. The healthiness of Mr. Finlaison's annuitants about 40 and 50 is one of the most remarkable features of his table: he observes (p. 58), that out of 10,000 persons at 23, 141 will die in a year, and 141 will die out of the same number at the age of 48; but at the age of 34 there will only die 124. The curve marked by obelisks, +, in the diagram, will show the comparative progress of mortality in this system; which, however valuable the data may be, appears to exhibit too many novelties, if not anomalies, to be generally adopted with confidence; while the line of crosses, ×, representing the tontine of Deparcieux, will serve to show how little difference the lapse of a century has made in the results of these two similar cases.

I shall conclude, my dear Sir, with a comparison of the climacteric years, as they may be called without impropriety, in which the greatest numbers of adults die, as taken from different tables.

I sincerely hope that these considerations may help to undeceive the too credulous public, who have of late not only received some hints that tend to insinuate the probability of an occasional recurrence of a patriarchal longevity, but who have been required to believe, upon the authority of a most respectable mathematician, that the true and unerring value of life is not to be obtained by taking an average of various decrements, but by adopting the extreme of all conceivable estimates, founded only on a hasty assertion of Mr. Morgan, and unsupported by any detailed report; an estimate which makes the grand climacteric of mankind in this country, not a paltry fifty-four, or the too much dreaded sixty-three, but no less than

EIGHTY-TWO! An age to which nearly one-sixth of the survivors at ten are supposed to attain!

CLIMACTERICS, OR GREATEST DECREMENTS.

Berlin, formerly	38	Breslau, 1695	61	Montpellier, 1782	67	Deparcieux	73
London, about 1733	40	Formula	63	Duvillard, France	67	Carlisle	74
Paris, formerly	40	Brandenburg	65	Sweden, 1762	68	Ackworth, 1752	75
Stockholm, 1762	42	Warrington, 1777	65	Chester, 1776	68	Kersseboom	77
London, 1764	43	Norwich, 1765	66	Sweden, 1785	69	Finlaison	78
London, 1815	46			Holycross, 1760	70	E. O. Mr. B.	82
Northampton, 1757	56						

Believe me, dear Sir,
Your faithful and obedient Servant,
THOMAS YOUNG.

Park Square, 28 Feb., 1826.

Mr. Morgan, the well-known writer on annuities, who was the nephew of Dr. Price and his successor in the administration of the Equitable Society, published in the 'Annals of Philosophy' for Oct., 1827, under the heading "Dr. Price and his Followers," the following reply to Dr. Young's argument.

"I know not, neither am I anxious to learn, for whom this civil appellation (*supra*, p. 373) is intended. If it merely refers to those theorems of Dr. Price which have been the subject of Dr. Young's animadversions in his late communication to the Royal Society, I should think it impossible that any person acquainted with the subject, would have the least difficulty in determining which of the two Doctors he should prefer to follow on this occasion.

"In the 66th volume of the Philosophical Transactions, Dr. Price gave sundry theorems for determining the values of annuities when the payments are made at shorter intervals than one year, and for that purpose proceeded on the same principles in investigating the values of the different payments with those universally adopted in regard to the values of such payments when they are made annually; for if $1l.$ increased by its interest for a year, or $\overline{1 + r}$, be the amount of $1l.$ in a year, $1l.$ increased by its interest for a shorter term will be its amount in that term. Supposing therefore such term to be $^1/a$th part of a year, $\frac{r}{a}$ will be the interest, and consequently $\overline{1 + \frac{r}{a}}$ will be the amount. The converse therefore of these expressions or $\frac{1}{1 + r}$ and $\frac{1}{1 + \frac{r}{a}}$ will be the present value of $1l.$ to be received at the end of a year, or at the end of $\frac{1}{a}$ th part of a year.—The series $\frac{1}{1 + r} + \frac{1}{\overline{1 + r}|^2} + \frac{1}{\overline{1 + r}|^3}$ ..... $\frac{1}{\overline{1 + r}|^n}$ is known to express the value of an annuity of $1l.$ for n years $= \frac{1}{r} - \frac{1}{r \, . \, \overline{1 + r}|^n}$. For the same reason, if $\frac{1}{a}$th part of $1l.$ be paid a times in each year,

the series $\frac{\frac{1}{a}}{1+\frac{r}{a}} + \frac{\frac{1}{a}}{\left(1+\frac{r}{a}\right)^2} + \frac{\frac{1}{a}}{\left(1+\frac{r}{a}\right)^3} \cdots\cdots + \frac{\frac{1}{a}}{\left(1+\frac{r}{a}\right)^{na}} = \frac{1}{r} - \frac{1}{r\cdot\left(1+\frac{r}{a}\right)^{na}}$ will express the value of 1*l*. per annum payable every $\frac{1}{a}$th part of a year for n years.

"In the preface to Taylor's Logarithms, Dr. Maskelyne assumes r to be the interest of 1*l*. for one *time*, and supposes the payment of the annuities to be made so many *times*. This is in fact the same as Dr. Price, and I believe every other person since his time have done, who have had a due knowledge of the subject. It necessarily follows that temporary annuities payable at shorter intervals than a year must be more valuable than annuities payable yearly; and in consequence, Dr. Price states the value of an annuity of 1*l*. payable half-yearly for five years at 4 per cent., to be 4·4913, and its value payable yearly to be 4·4518; or in other words, the two half-yearly fractions in any year being greater than the single fraction in the corresponding year, that is $\frac{\frac12}{1\cdot02} + \frac{\frac12}{(1\cdot02)^2}$ being greater than $\frac{1}{1\cdot04} \cdots \frac{\frac12}{(1\cdot02)^3} + \frac{\frac12}{1\cdot02^4}$ than $\frac{1}{(1\cdot04)^2}$ &c., it follows that the sum of the former must be greater than the sum of the latter. But instead of comparing the *sum* of the two half-yearly terms with the corresponding yearly term, Dr. Young compares the second of each half-yearly term with *half* the corresponding yearly term, or $\frac{\frac12}{(1\cdot02)^2}$ with $\frac{\frac12}{1\cdot04} \cdots \frac{\frac12}{(1\cdot02)^4}$ with $\frac{\frac12}{(1\cdot04)^2}$ &c., and by this means finds the discount taken half-yearly to be *greater* than the discount taken yearly; which if true, would make the value of an annuity payable half-yearly to be *less* than its value payable yearly, which is self-evidently wrong.*—Dr. Price is said to have fallen into error by 'adopting the legal restraints on usury as an essential step in his calculations.' Where does this appear? or what connection have those restraints with any calculations of the kind? I am certain that no such idea ever entered into the mind of Dr. Price, or of any person acquainted either with the doctrine of annuities or with the laws of this country. In order, however, to remove all difficulties and correct all errors on the subject, we are favoured with a much simpler process. 'An annuity of 1*l*. payable half-yearly (is said) to be equal to two annuities of ½—the one beginning at the end of the year—the other anticipated by half a year; and the value of this portion is greater than the other by half of the payment, that is by ¼,' if the annuity is payable quarterly, the excess will be ¾ the payment, or ⅜; or in other words, the excess in the first case will be ¼ year's purchase of the annuity; and in the second case it will be ⅜ year's purchase; nor does it appear to make any difference whether the annuity be for life or a term, or whether

* It may be easily proved that the value of the *second* half-yearly payment in each year is always less than the value of half the payment at the end of that year; and on the contrary that the value of the *first* half-yearly payment is always greater: and it may be further proved, that the difference between the values in the former case is always less than it is in the latter. In other words, that in any given year (n) the excess of $\frac{\frac12}{(1+r)^n}$ above $\frac{\frac12}{\left(1+\frac{r}{2}\right)^{2n}}$ is less than the excess of $\frac{\frac12}{\left(1+\frac{r}{2}\right)^{2n-1}}$ above $\frac{\frac12}{(1+r)^n}$.

the term be 5 or 50 years—or whether the rate of interest be 4 or 40 per cent., so that in *all cases* an annuity of 100*l.* worth 1000*l.* payable yearly, will be worth 1025*l.* if paid half-yearly, and 1012*l.* 10*s.* if paid quarterly. This is really setting all the rules of arithmetic at defiance, and requires no answer. By this indiscriminate mode of making the same additions to all annuities, it is admitted that the calculations in some instances become in appearance a *little paradoxical;* for the value of a reversionary annuity during one life after another, by making the same addition to the single as to the joint lives, becomes the same whether the payments are made yearly, half-yearly, or at shorter intervals: but it is well known that the additions to single and joint lives are different, and therefore that the values of all annuities in which single and joint lives are concerned must vary as the payments are more or less frequent.

"But I shall proceed no further with this subject, my motive in entering upon it having been to prove the truth of Dr. Price's solutions, rather than to expose the errors of those who do not understand them.

"The public have lately been overwhelmed with tables of the decrements of human life, formed either by amalgamating all the old tables into one heterogeneous mass, and thus giving the true probabilities of life in no place whatever, or by interpolating some of the decrements in one table into those of another; for which purpose a vast variety has been given of complicated and useless formulæ. But little or no advance has been made in determining more correctly the probabilities and duration of human life. The tables published in the Report of the Committee of the House of Commons are in general so incorrect, and some of them are even so absurd, as to be unfit for use; and serve only to encourage the popular delusion of the improved healthiness and greater longevity of the people of this kingdom.

"F. R. S."

Dr. Young's reply to this article forms the subject of No. LIX.—*Note by the Editor.*

No. LIX.

REMARKS ON THE

PRINCIPLE OF COMPOUND INTEREST.

In Reply to F. R. S.

From the Annals of Philosophy for November, 1827, vol. ii., p. 332, New Series.

If the interest of a hundred pounds for a day be a penny, it will be 365 pence for a year, according to the principles of simple interest.

But in all questions respecting pecuniary affairs extending to many years, and in all transactions respecting annuities, it becomes necessary to adopt the principle of compound interest, which allows a repeated investment of the capital and interest either yearly or monthly, or at shorter periods, without any limit: although this principle can only be employed in commerce under particular restraints imposed by the laws of usury.

It is lawful, for instance, to receive $\frac{5}{365}$*l.* in common years, if not in leap years, each day that 100*l.* is in the hands of a borrower: but at the end of the year, it is only lawful to receive 5*l.* for the use of the 100*l.* without any interest on the interest.

" Dr. Price and his followers" appear to consider the two supposed transactions as regulated by the *same rate* of interest.

The sense in which Dr. Young has understood the "*same rate*" comprehends the supposition of the possibility of laying out the interest from day to day, to be improved at compound interest; by means of which the 5*l.* would receive an addition of about half a year's simple interest on itself, or about half a crown, at the end of the year: making a yearly interest of about 5*l.* 2*s.* 6*d.*; which he considers as more correctly the

same rate with $\frac{1}{365}$*l.* a-day. If this is erroneous, Dr. Young is in the wrong.

It is allowable to use the word *same* in either sense, provided that the definition be borne in mind: but when the definition is forgotten, the confusion may lead to errors in practice. It has been asserted, for instance, that the value of a perpetual annuity payable yearly is exactly equal to that of the same annual sum supposed to be paid "momently" in equal portions: because this result is obtained by supposing the annual interest to be divided through the moments, according to the principle of simple and not of compound interest: and in this manner mathematicians, even of *deserved reputation*, have been able to convince themselves of the truth of a paradox so revolting to common sense. The fact is, that the obviously greater value of an annuity beginning immediately, and payable hourly or daily at the option of the receiver, is reduced, in their method of computation, by the virtual increase of the discount, to the bare value of an equal annuity of which the payments are all accumulated at the *ends* of the respective years; a change which certainly could not be a matter of indifference either to the payer or to the receiver, and neither of them "would have the least difficulty in determining which of the two Doctors he should prefer to follow," though their *determinations* might be somewhat at variance.

Dr. Young has not asserted that this difference is the same in *all* cases of annuities; although he has taken for granted that it is the same in annuities on lives as in perpetual annuities, because the present value of the remotest possible payments is in both cases evanescent.

The objector has certainly not understood the nature of the argument by which Dr. Young has attempted to prove the inaccuracy, or rather the impropriety, of Dr. Price's estimation of the *identity* of the rate of interest. The tenor of that argument is, that when Dr Price supposes he is reckoning on two annuities at the same rate of interest, he is in fact employing different rates: for that the discount on 10 shillings, receivable at a certain moment, as a payment of the half-yearly annuity, *is* greater, *in the computation*, than the discount on 10 shillings

receivable at the same moment as half of an annual payment; and therefore that the rate of interest cannot properly be called *the same* in the two cases. The objector makes Dr. Young assert that the discount *ought* to be greater when the payment is half-yearly; which is quite a different question, because this comparison relates to one half of the whole number of half-yearly payments only.

When the sense of an author is mistaken, it is easy to triumph over the supposed absurdity of his conclusions: and in *this manner* your correspondent has perfectly succeeded in exposing the errors of those who do not *understand* Dr. Price's solution! With the greater part of his last paragraph, however, I fully agree, though many might be inclined to oppose to him the high authority of Mr. Morgan, whose testimony, though somewhat vague, seems greatly calculated "to encourage the popular delusion of the improved healthiness and greater longevity of the people of this kingdom," which the objector seems so much to deplore.

F. R. S. L.

Waterloo Place, 2nd October, 1827.

Mr. Morgan replied briefly to Dr. Young in the January No. of the 'Annals of Philosophy' for 1828.—*Note by the Editor.*

No. LX.

A LETTER TO WILLIAM MORGAN, ESQ., F.R.S.,

ON THE EXPERIENCE OF

THE EQUITABLE SOCIETY.

From the Annals of Philosophy for November, 1828, vol. iv., p. 339, New Series.

DEAR SIR,

HAVING unfortunately failed, on some former occasions, of fully comprehending the meaning of your expressions, I earnestly entreat your attention to a few remarks on your late statement of the Experience of the Equitable Society, in order that you may correct, if possible, the exaggerated conclusions which appear to me to be fairly deducible from the numbers that you have published; for I have no doubt that you will unite with me in sincerely deprecating the dangerous consequences that would result from the hasty adoption of these conclusions in the practice of life assurance, although they may still be very useful as cautions deserving the attention of the granters of annuities.

Your table, lately published, stands thus:—

Age.	Number.	Died [annually.]	That is, One in *
20 to 30	4720	29	163 = 188 − 25
30 to 40	15951	106	150 = 185 − 35
40 to 50	27072	201	135 = 180 − 45
50 to 60	23307	339	69 = 124 − 55
60 to 70	14705	436	34 = 99 − 65
70 to 80	5056	219	17 = 92 − 75
80 to 95	701	99	7 = 94 − 87

[View of the Rise and Progress of the Equitable Society, 8vo., 1828, p. 42.]

1. I have first to observe, that the numbers of the column marked with an asterisk, which vary from 188 to 92, ought all, according to Halley's earliest hypothesis, to be 100; and according to Demoivre's correction of that hypothesis, to be 86 only. The mean of the numbers here computed is 137: and nearly in the proportion of 86 or 87 to 137 does the expectation of life, as exhibited by this table, exceed the estimate of the Northampton table; a result not materially differing from the proportion of 2 to 3, which you assign.

2. But a more remarkable peculiarity of the decrements, or rather the decremental quotients, derived from your table, is the regularity which is observable in their progress after the period of middle life; each of the numbers, which express them, being precisely or very nearly the half of the preceding number. Thus, disregarding fractions, we have $\frac{135}{2} = 67$ for 69, $\frac{69}{2} = 34$, $\frac{34}{2} = 17$, and $\frac{17}{2} = 8$ for 85, which is equivalent to 7 at $87\frac{1}{2}$.

3. We may therefore continue this series with perfect confidence, until the whole number of lives is exhausted, taking the annual decrement at 95, $\frac{1}{2}$, at 105, $\frac{1}{4}$, and at 115, 1, which may be supposed to be a sufficient age for the termination of our computations.

4. The decremental quotient in your table, $-\frac{\Delta z}{z}$, is very nearly $\frac{1}{2^y}$; y being $\frac{115 - x}{10}$, x the age, and z the number living; for this expression gives us, from 25 to 85, 512, 256, 128, 64, 82, 16, 8; and if we wish to modify the formula, we may make it more generally $\frac{-\Delta z}{z} = a^y$, and $y = b - cx$; so that the computation might be adapted to the earlier ages, if we had sufficient documents for the purpose.

5. We might at once form a table of mortality from the

quotients thus compu'ed, proceeding downwards from a single life at the age of 115; but it will be much more convenient, and perhaps equally accurate, to employ the method of fluxions.

6. Since $\frac{-\Delta z}{z} = a^y \Delta x$, Δx being $= 1$, $y = b - c\,x$, and $\Delta y = -c\,\Delta x$, we have $-\frac{\Delta z}{z} = a^y \cdot \frac{\Delta y}{-c}$; whence, substituting, as usual, $\frac{d\,z}{d\,y}$ for $\frac{\Delta z}{\Delta y}$, we have the equation $\frac{d\,z}{z} = a^y \cdot \frac{d\,y}{c}$, of which the fluent is $\text{h l}\, z = a^y \cdot \frac{1}{c\,\text{h l}\, a} + f$; which becomes, for the values $a = \frac{1}{2}$ and $c = \frac{1}{10}$, $f - \frac{10}{2^y \text{h l}\, 2}$, or $\text{h l}\, z = f - \frac{14{\cdot}42695}{2^y}$; which, when y is 11·5, becomes $f - {\cdot}0049926$; and if we suppose the number born to be 100,000, $11{\cdot}5129254 = f - {\cdot}0049926$, and $f = 11{\cdot}5179180$, and $\text{h l}\, z = 11{\cdot}517918 - \frac{14{\cdot}42695}{2^y}$, y being $= \frac{115 - x}{10}$. When $y = 0$ and $z^y = 1$, $\text{h l}\, z = -2{\cdot}90903 = \text{h l}\, \frac{1}{11}$, so that about one in a million only would survive at 115.

7. It is obvious that, according to this formula, the value of z can never become wholly extinct, and that a population may be imagined great enough to have an individual living at any given age; but notwithstanding Mr. Gompertz's ingenious speculations on patriarchal longevity, it can scarcely be admitted that the analogy is sufficiently strong to justify such a conclusion respecting more modern times; to say nothing of the population of the whole world as a limit which would require to be considered.

8. The results of the formula are exhibited in the following table, in which they are compared with Mr. Babbage's table of the Equitable Experience, with the Carlisle table, and with the table published in the Philosophical Transactions for 1826.

Age.	Living.	$B\times 2$	$C\times 2$	$Y\times\frac{1}{10}$	$N\times\frac{10}{9}$
(0	10000)				
(15	9900)				
(25	9761)				
(35	9490)				
45	8970	9612	9454	10535	10827
50	8561	8910	8794	9263	9523
55	8014	8060	8146	7928	8160
60	7299	7084	7286	6543	6793
65	6396	6048	6036	5130	5440
70	5310	4974	4802	3733	4107
75	4075	3876	3350	2432	2773
80	2808	2714	1906	1336	1563
85	1654	1302	890	571	620
90	785	340	284	177	153
95	26	40	60	31	13
100	6·1	0	18	7	0
105	·7		0	1	
110	·04			·3	
115	·01			0	

9. I am at a loss to understand how you will be able to reconcile the numbers of the first column of this table, with the opinion that "the experience of the Equitable Office confirms the accuracy of the Northampton table," which is represented by the fourth column, on the supposition that a given number of individuals about 55 is to be compared. From this age, and as far as 85, the first column certainly represents the numbers of your table, if I have not mistaken their import; but the formula may readily be made to extend with equal accuracy to ages somewhat above this limit, as well as to an earlier period. We may take, for example, 105 for the age at which the decremental quotient, indicating the *rate* of mortality, becomes = 1, and make it 150 at 35; it must then be $\sqrt{150}$ at the intermediate age of 70; and supposing $a^{70} = \frac{1}{150}$, $\frac{1}{a} = 1{\cdot}0742$, b being $= 105$, and $c = 1$, and the fluent becomes $= f - \frac{1}{hl\,1{\cdot}0742}\cdot\frac{1}{a^y}$, y being $105 - x$, or $f - \frac{13{\cdot}974}{a^y}$; and $\log a = {\cdot}031087$, and for 10000 at birth, $f = 9{\cdot}218$; which

gives at 95, 11 instead of 26, and at 55 7207, a number nearer to the truth than the former.

10. If, happily for the welfare of mankind, it should hereafter appear that any firm reliance ought to be placed on these conclusions, or if the formula could be any otherwise modified so as to serve for the purposes of calculation, it might be made to afford essential assistance in determining the values of two or more joint lives; and by means of a proper table of fluents, the labour would be little greater for combinations of lives than for single ones, since the sums of the fluents would represent the products of the quantities to be combined; and a single table might be computed, which would render the integration of the fluent of $e^{a^y} dy$ a matter of little difficulty. But such an improvement would at present be premature.

While results like these, however, are fairly deducible from the face of the evidence that you have laid before the public, you must allow, my dear Sir, that any government granting annuities would be highly culpable in reckoning on values of human life like those which are represented by the Northampton tables; and that any private office has a right to expect, beyond such a valuation, a fair percentage for the payment of their unavoidable expenses. On the other hand, I do not see how it is possible for any assurance office, not returning a large share of their profits, to satisfy the public that their terms are reasonable, without acting most improvidently for their own interests. Such offices as the Equitable are exempt from these objections; and I have not the least doubt of the judgment and integrity with which you have long conducted the business of that society, nor of the impropriety of calling on a private body to adopt any other regulations than those which are approved by its members. But, as a man of science, it is natural to hope that you will be ready to allow other men of science to partake in the fruits of your researches, and that you will be desirous of vindicating yourself from all possible suspicion of ambiguity and of inconsistency.

I am, dear Sir, with great respect, yours, &c.

* * * *.

Waterloo Place, Oct. 13, 1828.

No. LXI.

PRACTICAL COMPARISON OF THE DIFFERENT

TABLES OF MORTALITY.*

In a Letter to Sir Edward Hyde East, Bart., M.P., F.R.S.

From Brande's Quarterly Journal for 1828, vol. xxvi. p. 342.

My dear Sir,

I had the honour of addressing to you, a few years since, an investigation of the value of human life, which was published in the Philosophical Transactions for 1826: and I had then occasion to employ a formula for expressing the annual decrements of life at all ages, in such a manner as to serve sufficiently well for the intended purpose, of *harmonizing* the mean standard table, of which I had obtained the basis from a comparison of various documents. This formula would have been much too complicated for anything like a direct introduction into the detail of calculation: but I have lately had the good fortune to discover some simpler expressions, which are capable of being extensively applied, with great convenience, to different cases occurring in the practice of Insurance, and which may also be readily adapted to a variety of tables of mortality, so as to afford a far nearer approach to the results belonging to each, than could be obtained from calculations derived from any other tables; and will frequently indeed be more likely to represent the true law of nature at each place of observation, than the actual records of a limited experience for each particular year throughout life.

* Dr. Young had accepted the offices of actuary and medical referee to the Palladium Life Insurance Society, and in this capacity was extensively engaged in the practical application of the theory of insurances and annuities on lives.—*Note by the Editor.*

2. The great computer Demoivre employed, on different occasions, two different hypotheses respecting the mean value of life: and each of these has its advantages in particular cases. The first was the *arithmetical* hypothesis, supposing, for instance, that out of 100 or of 86 persons born together, 1 shall die annually till the whole number be exhausted. The second was the *geometrical* hypothesis, as, supposing that 1 in 50, or in 100, of the living at any age shall die within a year: a law which seems somewhat to approach to that of nature in extreme old age.

3. I have lately added to these, from examining a report of the experience of the Equitable Assurance Office, a third hypothesis, which may be called the *exponential;** the proportional mortality appearing to be represented by a geometrical progression of divisors; so that we may suppose the divisor to be doubled once in every ten years that the age falls short of 115; while in the Northampton table, which approaches very near to the law of the arithmetical hypothesis, the divisor requires to be doubled more nearly once in 22 years.

4. The exponential hypothesis affords us, as I have shown by an example, a ready mode of computing the number of survivors at a given age, as required by the supposed law of the divisors; but if we proceed to compute by it the expectation of life, or the value of an annuity, it leads, in the simplest cases, to a transcendental quantity, which has long served for the amusement or for the torment of the most refined mathematicians, under the name of a logologarithmic integral, without having been rendered the more manageable by all their elaborate investigations.

5. Still less would it be practicable to make any use of an additional exponential term, which might be made to express with great accuracy the decreasing mortality of early infancy and childhood. A difficulty nearly similar occurs also in computing from an expression which I had deduced from the equable variation of the value of an annuity under certain circumstances; a property which I have lately employed, as you will

* Supra, No. LX., p. 386.

recollect, for facilitating the valuation of out-standing policies for insurance. This formula for the decrement was $\frac{a+x}{b-x}$, which leads to the same hyperlogarithmic series as the exponential hypothesis.

6. We may form a correct conception of the character of the exponential hypothesis by laying down, in the diagram of my paper in the Transactions, the numbers of the table that I have published in my letter to Mr. Morgan, taking $\frac{2}{5}$ of the quinquennial differences for the comparative annual mortality; and it will be found that the curve thus obtained approaches, in its general appearance, surprisingly near to that of the Carlisle table, and considerably resembles the curves of Deparcieux and of Finlaison, especially between the ages of 40 and 80.

7. But something much more simple than this is required for practical purposes; that is, if we attempt to apply a formula to the detail of our computations; and we may exhibit the basis of such a formula to the eye by drawing a straight line from the age 0 to the highest point of the Carlisle curve, and continuing it to the age 85 or 90; and it will be obvious, from inspection, that a triangle like this approaches much nearer, between 10 and 80, to the character of all the rapidly ascending lines of Carlisle, Finlaison, and Deparcieux, than either Demoivre's horizontal line, or the slightly irregular curve of Northampton; and, from the employment of the area of the triangle, the law derived from it may be called the *quadratic* hypothesis.

8. In other words, we find that many of the modern tables appear to indicate, instead of a uniform decrement of life throughout the full period of vitality, a decrement nearly proportional to the age itself, and the quadratic hypothesis carries to its greatest possible extent the exaggeration of the *climacteric age*, as I have before denominated the age of the greatest mortality, which seems to have been actually creeping upwards for the last century, though less rapidly than has sometimes been supposed. Deparcieux made it 73, the Carlisle table

74, Mr. Finlaison 78, and Mr. Babbage's reduction of the alleged mortality of the Equitable Office 82, though my late computation upon the exponential hypothesis, derived from a corrected report, makes it only about 75. Now, the triangle of the quadratic hypothesis rises highest at its términation, and makes the supposed climacteric the year of unavoidable death to those who attain it. This is a peculiarity not very credible as a correct statement of a matter of fact, though it requires little or no correction when applied to the generality of results like those of the Carlisle tables; and, in other cases, its imperfections may probably be remedied without difficulty. On the other hand, the true climacteric of nature, as well as that of the geometrical hypothesis, is the year of birth, while in the arithmetical hypothesis there is no climacteric at any age. The mortality of London in 1815, and the Northampton table, approach to the arithmetical hypothesis as having no strongly marked climacteric after the year of birth, though they have each a maximum about the middle of the whole range of life. The abridged formula, which I suggested in my former letter to you, was $368+10x$ for the decrement of life, which is a combination of the arithmetical and quadratic hypotheses in equal proportions at the age of about 37, and expresses, as it was intended to do, a mean mortality between the old and the new observations; but it is more convenient to keep them separate in computation.

9. I shall now proceed to compare, with the different tables of Morgan, Milne, and others, the results of the arithmetical hypothesis, as expressed by $s=1-\frac{x}{c}$; and those of the formula $s=1-\frac{xx}{cc}$; which is the quadratic hypothesis; s being the comparative number of survivors at the age x, and c a constant quantity, which may be varied at pleasure from 80 to 100.

10. The first point of comparison is the annual mortality of the *arithmetical* hypothesis with that of the tables of Northampton, and the bills of mortality of London.

A. *Annual* mortality approaching to $\frac{1}{c} = .0115$.

Age.	Northampton × .015·	London, 1815 × $\frac{1}{8}$.
10	(.0078)	(.0048)
20	.0108	(.0046)
30	.0112	.0114
40	.0114	.0122
50	.0121	.0128
60	.0123	.0106
70	.0120	.0102
80	(.0094)	(.0054)
90	(.0013)	(.0025)

11. With the decrements of the *quadratic* hypothesis, we may compare those of the tables of Deparcieux and of Carlisle, and those which I have lately computed from the supposed experience of the Equitable Office.

B. *Annual* mortality approaching to $\frac{2x}{cc} = - \Delta s$, or $\frac{-\Delta s}{x} = \frac{2}{cc}$.

Age x.	$\frac{\Delta s}{x}$ Deparcieux.	$\frac{\Delta s}{x}$ Carlisle.	$\frac{\Delta s}{x}$ Equitable Ph. M.
10	(5.8)	(2.9)	
20	(2.6)	2.2	
30	(1.8)	1.9	
40	1.2	1.7	1.3
50	1.2	1.2	1.9
60	1.6	2.0*	2.7
70	1.6*	1.8	3.3*
80	1.4	1.4	3.0
90	(.3)	(.4)	1.8

The precise value of c is here disregarded, but it may be observed that it is nearly constant in each column towards the middle of life, and that it must be perfectly so for some time about the maximum, which is 60 or 70. The agreement is, however, less clearly seen in this comparison than by means of the diagram; the effect of the discordances and irregularities of observation being here most strongly marked, and disappearing as we pursue the computations further. The Northampton table, treated in this manner, gives a series of numbers always diminishing.

12. The whole number of the living at each age, exhibited in any tables, is computed from the annual decrements; and this number is next to be compared with the two hypotheses, omitting the years of infancy.

C. Number *living*, compared with $s = 1 - \frac{x}{87}$.

Age x.	s.	North. × .015.
10	.885	.851
20	.770	.770
30	.655	.648
40	.540	.545
50	.425	.429
60	.310	.306
70	.195	.185
80	.080	.070
90	.000	.007

D. *Living* compared with $s = 1 - \frac{xx}{cc}$.

Age x.	$c=87$.	$c=90$.	$c=93$.	Deparc. × .17.	Carlisle. × .155.	Eq. Off. × $\frac{1}{12}$.
10	.987	.988	.988	(1.021)	(1.001)	
20	.947	.951	.954	.961	.944	(.820)
30	.881	.889	.896	.858	.874	(.803)
40	.789	.802	.815	.763	.787	.770
50	.670	.691	.711	.674	.682	.713
60	.524	.556	.584	.542	.565	.610
70	.343	.395	.433	.359	.372	.442
80	.155	.210	.260	.138	.148	.234
90	.000	.000	.064	.014	.022	.065

It is obvious that the formulas approach, in both these comparisons, much nearer to the tables than in A and B. The column of Deparcieux is best represented by the divisor 87, at least from 25 to 80, and the same is true of the Carlisle table, except just about 60; while the supposed experience of the Equitable Office, after 40, agrees best with the divisor 90, or even 93.

13. The *expectation* of life, or the value of a life annuity without interest, is next to be determined for each hypothesis. The fluxion of the expectation is evidently equal to the fluxion of the age, multiplied by the chance of surviving to that age,

which is expressed by the quotient of the survivors, $\frac{s}{k}$, supposing k to be the initial number of the living at the given age, and the fluxion of the expectation is $\frac{s}{k}\,dx$, that is, $\left(1-\frac{x}{c}\right)\frac{dx}{k}$ or $\left(1-\frac{xx}{cc}\right)\frac{dx}{k}$, according to the hypothesis to be employed, and the fluents are $\frac{x}{k}-\frac{xx}{2ck}$ and $\frac{x}{k}-\frac{x^3}{3cck}$ respectively, taking the values from $s=k$ or $x=q$, the given age, to $x=c$, the extreme period of life assumed in the hypothesis.

14. Now, in the arithmetical hypothesis for $\frac{x}{k}-\frac{xx}{2ck}$, we have $\frac{q}{k}-\frac{qq}{2ck}$ and $\frac{c}{k}-\frac{cc}{2ck}$, the difference being $\frac{1}{k}\left(c-\frac{c}{2}-q+\frac{qq}{2c}\right)=\frac{cc-2cq+qq}{2ck}=\frac{(c-q)^2}{2ck}$. But $k=1-\frac{q}{c}=\frac{c-q}{c}$, and the expectation ε becomes $=\frac{c-q}{2}$, as is well known.

15. In the quadratic hypothesis, the two values of the fluent are $\frac{q}{k}-\frac{q^3}{3cck}$, and $\frac{c}{k}-\frac{c^3}{3cck}$, the difference being $\frac{3c^3-c^3-3c^2q+q^3}{3cck}=\varepsilon$: but $k=1-\frac{qq}{cc}$, $cck=cc-qq$, and $cckq=ccq-q^3$; whence $\varepsilon=\frac{2c^3-2c^2q-c^2kq}{3cck}=\frac{2cc}{3cck}(c-q)-\frac{q}{3}=\frac{2cc}{3}\cdot\frac{c-q}{cc-qq}-\frac{q}{3}=\frac{2cc}{3(c+q)}-\frac{q}{3}=\frac{c-q}{3}\cdot\frac{2c+q}{c+q}$, which varies from $\frac{2}{3}(c-q)$ to $\frac{1}{2}(c-q)$.

E. *Expectations*, compared with $\varepsilon = 43.5-\frac{1}{2}q$.

Age q.	ε.	Northampton.
10	38.5	39.8
20	33.5	33.4
30	28.5	28.3
40	23.5	23.1
50	18.5	18.0
60	13.5	13.2
70	8.5	8.6
80	3.5	(4.7)
90	.0	2.4

F. *Expectations* compared with $\varepsilon = \frac{2cc}{3(c+q)} - \frac{q}{3}$.

Age q.	ε, $c=87$.	Deparcieux.	ε', $c=90$.	$\frac{\varepsilon+\varepsilon'}{2}$.	Carlisle.
10	48.7	46.9	50.7	49.7	48.8
20	37.5	40.3	42.4	40.0	41.5
30	33.2	34.2	35.0	34.1	34.3
40	26.5	27.8	28.2	27.3	27.6
50	20.2	20.5	21.9	21.0	21.1
60	14.3	14.2	16.0	15.1	14.3
70	8.8	8.8	10.4	9.6	9.2
80	3.6	4.7	5.1	4.4	(5.5)
90	0.0	1.8	0	0	(3.3)

It appears, from this comparison, that we approach very near to the expectation of life at Carlisle, by taking the mean of ε and ε', or by making $c = 88.5$: and from 10 to 70 the formula appears to represent the mortality more correctly than the tables, which are extremely irregular in their differences, probably on account of the very small population on which the observations were made.

16. The only remaining determination to be considered, that is exempt from the effect of interest, is that of the *probability of survivorship* between two lives; a probability which is made up of the sum of the probabilities of survivorship for every year, or every portion of a year, throughout the full range of the life of the eldest; that is, the probability that the one will die within the element of time considered, while the other survives: so that the fluxion of the probability is $\frac{s}{k} \cdot \frac{ds'}{k'}$: k being the number surviving at the age of the eldest, q, and k' at that of the youngest, while s and s' represent the variable number of survivors.

17. In the arithmetical hypothesis we have constantly $ds = -\frac{dx}{c}$, and the fluxion of this probability is $\frac{s}{k} \cdot \frac{dx}{ck'}$; which is equal to the fluxion of the expectation ε, divided by ck', and the fluent being taken between the same limits $x = q$ and $x = c$ in both cases, it follows that $\frac{\varepsilon}{ck'}$ is, in this hypothesis, the value

of the probability that the younger life will fail first; and since $\varepsilon' = \frac{c - q'}{2}$, (14) and $k' = 1 - \frac{q}{c}$, we have $\frac{\iota}{ck'} = \frac{\iota}{2\iota'}$: a very simple consequence of this hypothesis, which appears hitherto to have escaped observation. The PROBABILITY, therefore, THAT THE YOUNGER OF TWO LIVES WILL FAIL BEFORE THE ELDER, IS EXPRESSED BY THE EXPECTATION OF THE ELDER DIVIDED BY TWICE THAT OF THE YOUNGER.* And it is ob-

* In a note to his excellent work on Annuities, p. 114, the late Mr. Francis Baily gave the same theorem, which is announced in the text "as having hitherto escaped observation." Dr. Young soon found out his mistake, and addressed a copy of his paper with the following note to Mr. Baily, which has been kindly communicated to me by Mr. De Morgan, who found it among his papers.

"DEAR SIR,

"YOU will see in page 8 that I had not read your paper with the attention it merited, as I could scarcely have forgotten your expression $\Psi = \frac{\iota}{2e}$, but I felt perfectly confident that Morgan could not have omitted to notice the approximation if it had been before observed, and I enquired no farther, so that I have allowed you grounds for a triumph, if you think proper to employ them.

"Believe me, very truly yours,

"*To Francis Baily, Esq., F.R.S.* "T. YOUNG.

5th Jan. 1829.

"It is only an hour ago that I *accidentally* cast my eye on your note."

This oversight of Dr. Young was made the subject of some disparaging observations in the Journals of that day, which induced Dr. Young to address another letter to Mr. Baily, which was published in Brande's Quarterly Journal for April, 1829.

"DEAR SIR,

"I HAVE been thinking, that, as an attempt has been made to direct the public attention to the oversight which I acknowledged to you the moment that I was aware of it, it might be right that I should ask you, whether there was any form, or any channel, by which you would like that I should publicly admit your undisputed claim to the discovery which I have lately printed in capitals as my own: and to assure you, that you cannot be more willing to point out such a proceeding than I should be to comply with your wishes.

"I have certainly given you an advantage, in printing, as a conspicuous part of my paper, a remark which you had only thought worthy of being inserted in a note; and for myself, the circumstance has been a little unlucky, not from any censure that may have been passed on me, as not having read every note even in the best book relating to the general subject of my essay; and still less from the contemptible suspicion of my having made a childish attempt to deck myself in borrowed plumes, and then to hold them so high that the slightest breath would blow them away: but because the occurrence tends to divert the attention of the reader from the essential subject of my essay; which, you may have observed, is, first to establish the superior convenience of *a good formula* in *preference to all tables* formed from a limited observation, for all ordinary cases of the valuation of annuities; that is, between the ages of ten and seventy: and secondly, to show, as I may hereafter do still more fully, that *a uniformly increasing decrement of survivors*, throughout the middle of life, will afford a value of mortality sufficiently near to the results of tables the most discordant among themselves, provided that the rate of increase be properly adjusted to the table.

"I am, dear Sir,

"Your faithful and obedient servant,

"Waterloo Place, 1 Feb., 1829." "* * * *

vious that by taking the several expectations as directly computed from the tables, this determination may be extended, as a good approximation, to the utmost limits of the observations.

G. *Probabilities* of survivorship, compared with $\pi = \frac{1}{87k'}$.

Ages.	π.	Northampton.	$\frac{1}{2\iota}$ N.
10, 20	.435	.415	.420
40, 50	.394	.394	.390
70, 80	(.206)	.300	.276
10, 50	.239	.206	.226
40, 80	.075	.102	.103
10, 80	.044	.044	.060

18. In the quadratic hypothesis, s being $1 - \frac{xx}{cc}$, we have $\mathrm{d}s = -\frac{2x}{cc}\mathrm{d}x$, and $\frac{2x}{kcc}\mathrm{d}x$ is the fluxion of the probability that a person of the supposed age will die at a certain time, which, for the age of the younger $x - p$, taking k' for k, becomes $2\frac{x-p}{cck'}\mathrm{d}x$, to be multiplied by $\frac{s}{k}$, the probability that the elder will survive, that is, by $\frac{1}{k}\left(1 - \frac{xx}{cc}\right)$: the product is $\frac{2}{cckk'}$ $\left(x\mathrm{d}x - p\mathrm{d}x - \frac{x^3}{cc}\mathrm{d}x + p\frac{xx}{cc}\mathrm{d}x\right) = \mathrm{d}\frac{2}{kk'}\left(\frac{xx}{2cc} - \frac{px}{cc} - \frac{x^4}{4c4} + \frac{px^3}{3c4}\right.$, whose integral taken from $x = c$ to $x = q$, becomes $\frac{2}{kk'}$ $\left(\frac{1}{2} - \frac{p}{c} - \frac{1}{4} + \frac{p}{3c} - \frac{qq}{2cc} + \frac{pq}{cc} + \frac{q^4}{4c^4} - \frac{pq^3}{3c^4}\right)$; that is, putting $\frac{p}{c} = p'$, and $\frac{q}{c} = q'$, $\pi = \frac{2}{kk'}\left(\frac{1}{4} - \frac{2}{3}p', -\frac{1}{2}q'^2, + p', q', + \frac{1}{4}q'^4, -\frac{1}{3}p', q'^3\right)$.

H. *Probabilities* of survivorship, compared with the quadratic hypothesis.

Ages.	π.	Carlisle.	$\frac{1}{2\iota}$ C.	Northampton.
30, 60	.172	.158	.209	(.230)
40, 80	.060	.074	.100	(.102)

The ages are here assumed very distant, in order to compare the extreme cases; otherwise the agreement would have been much more accurate: but it is obvious that the formula comes far nearer to the direct computation from the Carlisle tables than the value derived from the Northampton tables.

19. We are now to examine the consequences of the two hypotheses in cases which require the consideration of interest or discount, to be combined with that of the contingency of survivorship at each step. In order to represent such cases, we must multiply, as is well known, the fluxion of the contingency of payment by the power r^x, or rather r^{x-q}, for the value as referred to the age q, r being the present value of a unit payable at the end of a year: for instance $\frac{100}{104}$, if we reckon at 4 per cent. compound interest. But it must be remembered that in this mathematical sense of compound interest, the interest of £100 for a quarter of a year is no more £1, at 4 per cent., than it is £16 for 4 years; and if we wish to reckon at the rate of £$\frac{4}{365.25}$ for a day, we must necessarily make the interest something more than £4 for a year. In almost all cases occurring in practice, the difference of the two modes of considering the interest is half a year's purchase of an annuity, payable annually: but sometimes, for an annuity of a very short duration, a further correction may be required: the correction is, however, in all cases, very easily computed, and generally by taking the fluent half a period later, both at the beginning and at the end of the term.

20. The present value of an annuity on a single life may, therefore, be represented by $-\int r^{x-q}\frac{s}{k}\,dx$, since dx is negative; that is, in the arithmetical hypothesis $-\int \frac{r^{x-q}}{k}\left(1-\frac{x}{c}\right)dx = -\int \frac{r^{x-q}}{k}dx + \int \frac{r^{x-q}}{ck}\,x\,dx$; and the fluent must be taken as usual from $x = c$ to $x = q$.

21. The general theorem for fluents of this form is $\int a^x x^n \, dx = \frac{a^x}{h l a}\left(x^n - \frac{n x^{n-1}}{h l a} + \frac{n(m-1) x^{n-2}}{h l^2 a} \quad \frac{n(n-1)(n-2)\ x^{n-3}}{h l^3 a} + \ldots\right)$, or putting λ for $\frac{-1}{h l a}$, $= -\lambda a^x (x^n + \lambda n x^{n-1} + \lambda^2 n (n-1) x^{n-2} + \ldots)$, whence the present fluent becomes $+ \frac{\lambda}{k} r^{x-q} - \frac{\lambda r^{x-q}}{ck}. (x + \lambda)$, λ being here positive, because a is less than unity; and putting q and c successively for x, we have $\frac{\lambda}{k} - \frac{\lambda}{ck}(q + \lambda) = \frac{\lambda}{k}\left(1 - \frac{q}{c} - \frac{\lambda}{c}\right) = \lambda - \frac{\lambda\lambda}{ck}$ and $\frac{\lambda}{k} r^{c-q} - \frac{\lambda}{ck} r^{c-q}(c + \lambda) = -\frac{\lambda\lambda}{ck} r^{c-q}$, the difference being $\lambda - \frac{\lambda\lambda}{ck}(1 - r^{c-q}) = \Lambda$, the present value of the annuity: and at 4 per cent. we have $\lambda = 25.497$, $\lambda\lambda = 650$, and if $\frac{1}{c} = .0115$, the formula becomes $\Lambda = 25.497 - \frac{7.475}{k}(1 - r^{c-q})$; the results of which agree sufficiently well with the Northampton table.

I. *Annuities* at 4 per cent. upon the arithmetical hypothesis.

Age.	Daily payments.	Annual payments.	Northampton tables.	Difference.
10	17.49	16.99	17.52	
20	16.56	16.01	16.03	+ .02
30	15.31	14.81	14.78	− .03
40	13.20	12.70	13.20	+ .50
50	12.02	11.52	11.26	− .26
60	9.75	9.25	9.04	− .21
70	6.99	6.49	6.36	− .13
80	2.56	2.06	(3.64)	

22. In the quadratic hypothesis, the fluxion is $-\int r^{x-q} \frac{s}{k} \, dx = -\int r^{x-q} \frac{dx}{k} + \int r^{x-q} \frac{xx}{cck} \, dx$ and the fluent $\frac{\lambda}{k} r^{x-q} - \frac{\lambda}{cck} r^{x-q} (x^2 + 2\lambda x + 2\lambda^2)$ which from $x = q$ to $x = c$ affords us $\frac{\lambda}{k} - \frac{\lambda}{cck}(q^2 + 2\lambda q + 2\lambda^2) - \frac{\lambda}{k} r^{c-q} + \frac{\lambda}{cck} r^{c-q} (cc + 2\lambda c + 2\lambda\lambda) = \frac{\lambda}{k}\Big(1 - \frac{qq}{cc} - \frac{2\lambda q}{cc} - \frac{2\lambda\lambda}{cc} + r^{c-q}\Big[\frac{2\lambda}{c}$

$+ \frac{2\lambda\lambda}{cc}\Big]\Big)$, or, since $k = 1 - \frac{qq}{c^2}$, $\Lambda = \lambda - \frac{\lambda}{k}\Big(\frac{2\lambda q}{cc} + \frac{2\lambda\lambda}{cc} - r^{c-q}\Big[\frac{2\lambda}{c} + \frac{2\lambda\lambda}{cc}\Big]\Big)$: that is, at 4 per cent and taking $c = 88.5$, and $\frac{1}{c} = .0113$, λ being $= 25.497$, $\lambda\lambda = 650$, and $\frac{\lambda}{c} = .28812$, $\Lambda = 25.497 - \frac{1}{k}\,(4.233 + .166q - 18.923\, r^{c-q})$.

K. *Annuities* at 4 per cent. upon the quadratic hypothesis.

Age	Daily payments.	Annual payments.	Carlisle tables.	Difference.
10	20.43	19.93	19.58	+ .35
20	18.93	18.43	18.36	+ .07
30	17.25	16.75	16.85	− .10
40	15.37	14.87	15.07	− .20
50	13.17	12.67	12.87	− .20
60	10.67	10.17	9.66	+ .51
70	7.40	6.90	6.71	+ .19
80	3.72	3.22	4.18	− .96
			Mean	− .04

It is obvious that a mean error so small and so subdivided is as likely to belong, in great measure, to the observations as to the computations.

23. It was my intention to proceed, in a similar manner, through the computations of annuities on two joint lives, and of the contingent reversions of survivorships: but the accuracy of the proposed formulas appears to be already abundantly demonstrated by the two last comparisons, and I shall confine myself, for the present, to the great remaining problem of *three joint lives*, the facilitation of which would be really a step of practical importance, even if we allowed the accuracy of the existing tables, which have been the most extensively employed for calculations of this kind.

24. The age of the eldest of three lives being x, and the ages of the two younger $x - p'$ and $x - p''$, the initial values of s, s', and s'' being k, k', and k'' respectively, when $x = q$; the

probability of the survivorship of the whole three, for any other values of x, will be $\frac{ss's''}{kk'k''}$, and the fluxion of the present value of the annuity will be $\frac{ss's''}{kk'k''} r^{x-q} \mathrm{d}x$. Now, $s = 1 - \frac{xx}{cc}$, $s' = 1 - \frac{(x-p')^2}{cc}$, and $s'' = 1 - \frac{(x-p'')^2}{cc}$, or neglecting c, which always accompanies x and p, till the end of the computation, $s' = s + 2p'x - p'p'$, and $s'' = s + 2p''x - p''p''$; whence $ss' = ss + 2p'xs - p'p's$, and $ss's'' = s^3 + 2p'xs^2 - p'p's^2 + 2p''xs^2 + 4p'p''x^2s - 2p'p'p''xs - p''p''s^2 - 2p'p''p''xs + p'p'p''p''s = s^3 - (p'p' + p''p'')s^2 + p'p'p''p''s + 2(p' + p'')s^2x + 4p'p''sx^2 - 2p'p''(p' + p'')sx$.

25. Hence it appears that this contingency comprehends that which belongs to the value of three equal joint lives, as well as those which relate to one life and to two, these latter being also complicated with the expression of the age and of its square: and it is obvious that the result may be reduced to the form $kk'k''\mathrm{A} = Q - (p'p' + p''p'')Q' + p'p'p''p''Q'' + 2(p' + p'')Q''' + 4p'p''Q'''' - 2p'p''(p' + p'')Q'''''$: all the quantities Q... being dependent on the oldest life only, and capable of being expressed in a table by as many single numbers for each age, to be afterwards combined according to the variations of the younger lives, as here expressed by the differences. The first three numbers might be readily obtained according to any given tables of observations from the tables of the values of *equal* joint lives already in existence, or they may be computed with the rest, from the formulas: the last of all is also subservient to the calculation of the value of survivorships.

26. In the first place, for the quantity Q, belonging to three equal lives, we have $-\int s^3 r^{x-q}\mathrm{d}x = -\int r^{x-q}\mathrm{d}x\,(1 - 3x^2 + 3x^4 - x^6) = \lambda r^{x-q}(1 - 3(x^2 + 2\lambda x + 2\lambda\lambda) + 3(x^4 + 4\lambda x^3 + 12\lambda^2x^2 + 24\lambda^3x^2 + 24\lambda^4) - (x^6 + 6\lambda x^5 + 30\lambda^2x^4 + 120\lambda^3x^3 + 360\lambda^4x^2 + 720\lambda^5x + 720\lambda^6)$; which, taken from $x = q$ to $x = c$, becomes $\lambda\ (1 - 3\ (q^2 + 2\lambda q + 2\lambda\lambda) + 3\ (q^4 + 4\lambda q^3 + 12\lambda^2q^2 + 24\lambda^3q + 24\lambda^4) - (q^6 + 6\lambda q^5 + 30\lambda^2q^4 + 120\lambda^3q^3 +$

$360\lambda^4 q^2 + 720\lambda^5 q + 720\lambda^6) - r^{c-q}[1 - 3(1 + 2\lambda + 2\lambda\lambda) + 3(1 + 4\lambda + 12\lambda^2 + 24\lambda^3 + 24\lambda^4) - (1 + 6\lambda + 30\lambda^2 + 120\lambda^3 + 360\lambda^4 + 720\lambda^5 + 720\lambda^6)]) = Q$: the numbers λ and q, wherever they occur after the first λ, being understood as divided by c.

27. The second quantity is $Q' = -\int r^{x-q}\, ss\mathrm{d}x$, ss being $= 1 - 2\frac{xx}{cc} + \frac{x^4}{c^4}$, or $1 - 2xx + x^4$; which gives the fluent $\lambda r^{x-q}(1 - 2(x^2 + 2\lambda x + 2\lambda\lambda) + x^4 + 4\lambda x^3 + 12\lambda^2 x^2 + 24\lambda^3 x + 24\lambda^4)$: and this from $x = q$ to $x = c$ is $\lambda(1 - 2q^2 - 4\lambda q - 4\lambda\lambda + q^4 + 4\lambda q^3 + 12\lambda^2 q^2 + 24\lambda^3 q + 24\lambda^4 - r^{c-q}[1 - 2 - 4\lambda - 4\lambda\lambda + 1 + 4\lambda + 12\lambda^2 + 24\lambda^3 + 24\lambda^4]) = Q'$: restoring c in its place.

28. For Q'' we have $-\int r^{x-q}\, s\mathrm{d}x$, as in the case of a simple annuity (22), $= \lambda\, r^{x-q}(1 - x^2 - 2\lambda x - 2\lambda)$ giving $\lambda(1 - q^2 - 2\lambda q - 2\lambda\lambda + r^{c-q}(2\lambda + 2\lambda^2) = \lambda(k - 2\lambda q - 2\lambda\lambda + 2\lambda r^{c-q}[1 + \lambda])$.

29. In the next place $Q''' = -\int r^{x-q}\, ssx\mathrm{d}x = -\int r^{x-q}(x\mathrm{d}x - 2x^3\mathrm{d}x + x^5\mathrm{d}x) = \lambda r^{x-q}(x + \lambda - 2(x^3 + 3\lambda x^2 + 6\lambda^2 x + 6\lambda^3) + x^5 + 5\lambda x^4 + 20\lambda^2 x^3 + 60\lambda^3 x^2 + 120\lambda^4 x + 120\lambda^5)$; which, from $x = q$ to $x = c$, gives $\lambda(q + \lambda - 2(q^3 + 3\lambda q^2 + 6\lambda^2 q + 6\lambda^3) + q^5 + 5\lambda q^4 + 20\lambda^2 q^3 + 60\lambda^3 q^2 + 120\lambda^4 q + 120\lambda^5 - r^{c-q}[1 + \lambda - 2(1 + 3\lambda + 6\lambda^2 + 6\lambda^3) + 1 + 5\lambda + 20\lambda^2 + 60\lambda^3 + 120\lambda^4 + 120\lambda^5])$.

30. For Q'''', derived from sx^2, we have $-\int r^{x-}(x^2\mathrm{d}x - x^4\mathrm{d}x) = \lambda r^{x-q}(x^2 + 2\lambda x + 2\lambda\lambda - [x^4 + 4\lambda x^3 + 12\lambda^2 x^2 + 24\lambda^3 x + 24\lambda^4])$; and this, when corrected, becomes $\lambda(q^2 + 2\lambda q + 2\lambda\lambda - q^4 - 4\lambda q^3 - 12\lambda^2 q^2 - 24\lambda^3 q - 24\lambda^4 - r^{c-q}[1 + 2\lambda + 2\lambda\lambda - 1 - 4\lambda - 12\lambda^2 - 24^3\lambda - 24\lambda^4]$.

31. Lastly, for Q''''', from sx, we have $- \quad r^{x-q}(x - x^3)\mathrm{d}x = \lambda r^{x-q}(x + \lambda - (x^3 + 3\lambda x^2 + 6\lambda^2 x + 6\lambda^3)$, which becomes $\lambda(q + \lambda - (q^3 + 3\lambda q^2 + 6\lambda^2 q + 6\lambda^3) - r^{c-q}(1 + \lambda - [1 + 3\lambda + 6\lambda^2 + 6\lambda^3])$.

32. Taking, for a single example, the value of three joint lives of 30 at five per cent., we have $q = 30$, $\frac{1}{c} = .0113$, $\frac{q}{c} =$

.339, $\lambda = 20.5$, $\frac{\lambda}{c} = .23165$, and $r^{c-q} = .0576$. By substituting these quantities in the expression $\frac{Q}{k^3} = \Lambda$, we have the value 11.37.

L. *Three joint* lives, at 5 per cent., compared with the Carlisle tables.

Common age.	Daily payments.	Annual payments.	Carlisle tables.	Northampton tables.
30	11.37	10 87	10.82	(8.50)

33. For the arithmetical hypothesis, the computation becomes still more simple, and requires no auxiliary tables beyond those which are universally known. The contingency for the three joint lives here becomes $\frac{s^3 + (p' + p'') s^2 + p'p''s:}{kk'k''}$, and the value, taking Λ', Λ'', Λ''', for the existing tabular values of 1, 2, and 3 lives at the age of the eldest, $\frac{k^2\Lambda''' + (p' + p'') k\Lambda'' + p'p''\Lambda'}{k'k''}$, p' and p'' being the excess of the elder above the two younger respectively, divided by c: we might also add half a year to the tabular numbers, and deduct it from the final result, if necessary.

34. The same simplification is applicable to two joint lives, the contingency becoming $\frac{s}{k} \cdot \frac{s'}{k'} = \frac{'}{kk'}\ s\ (s + \frac{p'}{c}) = ss + \frac{p's}{kk'}$ and the values $\frac{k\Lambda''}{k'} + \frac{p'\Lambda'}{ck'}$.

M. *Two joint* lives, at 4 per cent., from the equal lives.

Ages.	Approximation, N. T.	Particular tables. (Morgan.)
40, 20	10.929	10.924
60, 40	7.396	7.490
80, 70	2.951	2.757

N. *Three joint* lives, at 4 per cent., from the equal lives.

Ages.	Approximation, N. T.	Morgan, Table VI.
30, 20, 10	10.13	10.438
50, 40, 30	7.42	7.571
70, 60, 50	4.26	4.219

35. You will perceive, my dear Sir, that these examples sufficiently establish the accuracy of my method of computing, without immediate reference to tables, during the most important portions of life; and if it be found sufficient, it must be allowed to possess a decided superiority, in the facility with which any imaginable change in the value of life is introduced into the computation. This modification is very readily effected by changing the constant quantity c in either mode of computation, or by combining the results obtained for any particular case from both methods, in such a manner and in such proportions as may be thought most desirable. But it is obvious that, in making these combinations, there must still be ample scope for the exercise of sound judgment and discretion, aided always by personal experience and cautious reflection.

I am, my dear Sir,

Yours most sincerely,

* * * *

Waterloo Place, 17th Nov., 1828.

Postscript.—The *quadratic hypothesis* may easily be accommodated to any table like my own, of which the decrements are nearly expressed by the mixed formula $368 + 10x$, considering the fluents between the given age and the time of total extinction only: and the same formulas will comprehend the *arithmetical hypothesis* as a particular case.

No. LXII.

PRACTICAL APPLICATION OF

THE DOCTRINE OF CHANCES

TO THE SUBDIVISION OF RISKS.

From Brande's Quarterly Journal for 1826, vol. xxii. p. 84.

It is well known to those who have studied the theory of chances, that where the magnitude of a risk may be divided into an indefinite number of parts, it is possible to confine the probability of the occurrence of any given excess or deficiency in the result, above or below its mean value, within any given limits; and it is of great practical importance to ascertain, both how far the subdivision ought to be carried, with regard to insurances, and what are the inconveniences to be apprehended, from the admission of occasional deviations from the general rule.

We may take, for example, the case of an office having undertaken 1000 risks of £5000 each, at a fair premium of 1 per cent. per annum, so as to have an annual income of £50,000, to be expended in the payment of losses. It would be very inconvenient to such an office to be liable to the frequent occurrence of an annual loss amounting to twice its income; and in fact the chance of such an occurrence is only about 1 in 333.

If the whole income of such a society were derived from 500 risks of £10,000 each, the chance of a similar loss would be about 1 in 32, which is a degree of probability that ought not voluntarily to be incurred without some very powerful motives.

Still less would it be justifiable to engage the whole responsibility of such a society in 100 risks only of £50,000 each;

the chance of losing at least £100,000 in a year becoming, in this case, somewhat more than 1 in 4.

But it is equally demonstrable that a very exact adherence to the *precise amount* of the risk, which is thought most eligible, cannot be considered as essential to the reasonable security of the society. Thus, admitting the propriety of confining the risks in general to 1000 of £5000 each; if a few risks, not more than 10 for instance, of double the amount, were added to the number, the chance of losing an additional year's income, or £50,000, by the failure of 5 of them, would be no more than 1 in about five millions: if 20 double risks were accepted, the chance of losing 5 would still be only 3 in two millions: and if the same number of single risks were rendered more hazardous, so as to require a double percentage; that is, if 20 risks of £5000 were accepted at 2 per cent., the chance of losing a year's income by these would be next to nothing, and that of losing £30,000 would be only 2 in a million.

It may be observed, that the increase of the per-centage, on a risk which becomes more hazardous, on any given sum, brings with it an adequate remedy, in the increase of income, as well for the combinations of risks, as for the single adventure, without the necessity of any diminution of the amount of the separate risks.

Taking, for example, the extreme case of a risk of cent. per cent. on 10 adventures of £5000 each; it is *certain* that the *whole* income will be lost, but impossible that the loss should at all *exceed* the income.

If the same income were derived from 10 adventures of £50,000 at ten per cent., there would be about one chance in three that nothing would be lost, and a little more than one in four that at least twice the income would be lost.

With 100 risks of £5000 each, at 10 per cent., the chances of losing twice the income are only 1 in 505; the danger of exceeding the limit being less than one hundredth part of the danger incurred from the same number of risks of £50,000 at 1 per cent., and little more than half as great as the danger with 1000 risks at one per cent.

It may also be inferred, from the examples here computed,

that when an office is well established, and is prudently conducted, there can be no practical necessity for having in readiness a deposit, exceeding the amount of the annual income, which is supposed to be £50,000; it is also obvious that a similar sum would be amply sufficient for all the contingencies that are at all likely to occur in such an office, from the time of its foundation, during the gradual accumulation of its transactions, to the supposed extent.

The calculations on which these assertions are founded, are subjoined at large in a Note.

The same mode of computation is equally applicable, whether the whole of the contingencies concerned are of the same, or of different kinds, provided that the risks be fairly estimated; and the result would by no means be affected by their dissimilarity. There is a common prejudice, that it is disadvantageous for an office to take a single risk of any particular description; and it is sometimes said, that if the adventure should happen to be unsuccessful, there would be no possible compensation from others of the *same* kind: there is, however, just the same chance that it would be compensated by others of a *different* kind; and if it were not, a fair price has been received for the responsibility, which it was worth while to incur from the probability of escaping, provided that the magnitude of the adventure was not too formidable; although the impression made *on the mind* by a singular event, is sometimes stronger and more disagreeable than by a more ordinary one. No person has ever attempted to assign, by calculation, what the precise disadvantage is that attends on a singularity of risk; nor has it ever been defined what degree of singularity there must be, in order to render a risk ineligible, except it involved a practical difficulty in appreciating it. It cannot be determined, for example, whether or no an insurance on the life of a negro, a mulatto, or an Indian, for an *adequate* premium, is a different *kind* of risk from an insurance on the life of a European; and if the affirmative were asserted, it would be difficult to show that a single insurance from a given town or county ought not to constitute an objectionable risk for a similar reason. And if it were possible to distinguish any imaginable diversity in the kinds of risks, so

as to have, for example, 1000 kinds; and if 1000 equal risks of the separate kinds were at first undertaken by 1000 different offices, it is manifest that if each of these 1000 offices, instead of confining themselves to the same kinds of risks, exchanged, in the second place, one of its risks with each of the other 999, so as to have all heterogeneous, instead of all homogeneous risks, the profits and losses of each office being supposed to be fairly balanced on the original supposition, they would remain equally balanced under the new distribution; nor is there any thing in the supposed change of combination that could affect the liability to greater or less deviations: at least, if there were any accident that could lead to such an inconvenience, it would probably be rather more likely to be diminished by the combination of heterogeneous elements, than by the confining the separate societies to their primitive homogeneous undertakings, which might possibly partake more of the nature of an undivided single risk, in the liability to inconvenient fluctuations.

7, Waterloo Place, 24 Aug., 1826.

There is appended to this Memoir in the original, a note, containing a well known formula for computing the risks referred to, applied to several numerical examples: it has not been thought necessary to reprint it.—*Note by the Editor.*

No. LXIII.

ADDENDUM TO THE ARTICLE ON

ANNUITIES.*

From the Supplement to the Encyclopædia Britannica.

As an addition to the article Annuities, we beg to insert here an expeditious method of calculating the values of annuities on single or joint lives, from any tables or bills of mortality, with sufficient accuracy for all practical purposes.

We must begin by determining the mean complement of life, according to the average number of deaths during a certain period, which must vary according to the nature of the proposed calculation; being shorter as the rate of interest is higher and as the number of lives concerned is greater; but not requiring to be very accurately defined. If the rate of interest be r, we must find the time in which the number of deaths is expressed by the fraction $\frac{3}{r+3}$ of the whole number of survivors at the given age, for a single life: for two lives, the fraction must be $\frac{3}{r+5}$, and for three, $\frac{3}{r+7}$; and, in each of these cases, the time determined from the age of the oldest life must be employed for finding the complements of both the others.

Having thus calculated the complements for each of the ages, we may, in most instances, save ourselves the trouble of further computation, by employing tables of the value of annuities on one and two lives, according to Demoivre's hypothesis. For this purpose, we have only to subtract the complement from 86, and we obtain an equivalent life on this hypothesis. If we take, for example, the age of 20, the number of

* Dr. Young was not the author of the article on Annuities in the body of the work, with which this Addendum has no essential connection.—*Note by the Editor.*

survivors in the Northampton tables is 5132: and for a single life, at 3 and at 6 per cent. we must find the time at which they are reduced $\frac{3}{6}$ and $\frac{3}{9}$ respectively; that is, to about 2566 and 3421: now at 54 and 43, the numbers are 2530 and 3404; and $\frac{5132 \times 34}{5132 - 2530} = 67 \cdot 07$, and $\frac{5132 \times 23}{5132 - 3404} = 68 \cdot 3$; whence the equivalent ages in Demoivre's tables are 18·93 and 17·7, giving 18·62 and 12·43 for the value of the annuity; while Dr. Price's table, deduced from the actual decrements at all ages, gives 18·64 and 12·40.

The utility of this mode of calculation will be still further illustrated by a comparison of the very different values of lives, as indicated by different tables. Taking, for example, the age of 30, and the interest at 5 per cent., we may find the value of the annuity, by this approximation, in different situations, for which correct tables have been published by Dr. Price, and may thence infer how much nearer it approaches to the truth than the generality of the results approach to each other:

London, 1730	Compl.	41·52	Value	11·22	Dr. P.	11·6
Northampton		57·05		13·09		13·07
Sweden, males		67·93		14·04		13·89
Deparcieux		71·11		14·28		14·72
Sweden, females		75·60		14·58		14·27

According to the bills of mortality of London for 1815, out of 9472 survivors at 30, 5573 lived to 50, and this is near enough to $\frac{3}{8}$ for our purpose: hence the complement is 48·58, and the value of an annuity at 5 *per cent.* 12·16 years' purchase. Where the age is much greater, the approximation is somewhat less accurate, though not often materially erroneous; thus, at 70, the values, according to the Northampton tables, at 3 and 6 per cent. are 6·23 and 5·35, instead of 6·73 and 5·72 respectively.

In the values of joint lives, there is more difference, according to the different tables employed, than in those of single lives: thus, at 30, the value of an annuity, at 4 per cent. on a single life, differs at Northampton, and in Sweden, in the pro-

portion of 14·78 to 16·00, or of 12 to 13; but for two joint lives at 30, in that of 11·31 to 12·96, or of 7 to 8; and for three lives, the disproportion would be still greater.

In the absence of Demoivre's tables, or for cases to which they do not extend, it becomes necessary to calculate the value of the annuity for each particular instance. Calling then the complement, as already determined, a, the number of survivors after x years will be represented by $a - x$, and the present value of any sum to be paid to each of them by $av^x - xv^x$, v being the present value of a unit payable at the end of a year: and if we suppose such payments to be made continually, their whole present value may be found by multiplying this expression by the fluxion of x, and finding the fluent, which will be $-pv^x$ $(a - x - p)$, p being $= -\frac{1}{\text{HL}v}$, or the reciprocal of the hyperbolical logarithm of the amount of a unit after a year. When x vanishes this fluent becomes $-p(a - p)$, and when $x = a$, p^2v^a; the difference divided by a, gives the present value of the annuity $p - \frac{p^2}{a} + \frac{p^2v^a}{a}$; from which, when the annuity is supposed to become due and to be paid periodically, we must subtract in all cases half a payment; that is, $\frac{1}{2}$ for yearly payments, and $\frac{1}{4}$ for quarterly; and if, at the same time, we choose to assume that money is capable of being improved by laying out the interest more frequently than once a year at the given rate, we must alter the value of v accordingly.

For two joint lives, the complement of the elder, determined from the fraction $\frac{3}{r+5}$, being a, and that of the younger, deduced from the deaths in an equal number of years, b, we have for the binary combinations of the survivors, after x years, $(a - x)(b - x)$, and the fluent will be $-pv^x(ab - (a + b)$ $(x + p) + x^2 + 2px + 2p^2)$, which, corrected and divided by ab, gives the value of the annuity $p - \frac{p^2}{ab}(a + b - 2p) -$ $\frac{p^2v^a}{ab}(a - b + 2p)$; and this, with the deduction of half a payment, agrees with the tables calculated on Demoivre's hypothesis, taking the same complements of life.

But for three lives we have no such tables, and this method

of calculation becomes therefore of still greater importance. Employing here the fraction $\frac{3}{r+7}$ for the oldest life, we must determine the complement a for this life, and those of the two younger, b and c, from an equal period. The combinations will then be $(a - x)(b - x)(c - x) = abc - (ab + ac + bc)x + (a + b + c)x^2 - x^3$, which we may call $d - ex + fx^2 - x^3$; hence the fluent is found $-pv^r\left(d - e(x + p) + f(x^2 + 2px + 2p^2) - (x^3 + 3px^2 + 6p^2x + 6p^3)\right)$ this, when x vanishes, becomes $-p(d - ep + fp^2 - 6p^3)$, and calling this $-pg$, the corrected fluent will give the value of the annuity $\frac{pg}{d} - \frac{pv^a}{d}(g - ea + fa^2 + 2fpa - a^3 - 3pa^2 - 6p^2a)$. Thus, if the ages are 10, 20, and 30, and the rate of interest 4 *per cent.*, we find, in the Northampton tables, the survivors at 30 4385, $\frac{8}{11}$ of which are 3199; and at 46, the survivors are 3170; whence a = 57 7, and b and c found also from periods of 16 years after the respective ages, are 68·5 and 91·7. Calculating with these numbers, we find the value of the annuity 10·954 − ·5 = 10·454. Dr. Price's short table gives it 10·438; and Simpson's approximation from the tables of two joint lives 10·563, which is less accurate in this instance, even supposing such tables to have been previously calculated.

It would, indeed, be easy to form, by this mode of computation, a table of the corrections required at different ages for Simpson's approximation, since these corrections must be very nearly the same, whether Demoivre's hypothesis or the actual decrements of lives be employed, both for the two joint lives, and for the correct determination of the three. But the value thus found would still be less accurate, with respect to any other place, or perhaps even any other time, than the immediate result of the mode of calculation here explained.

It may, perhaps, save some trouble to subjoin a table of the values of p and their logarithms.

3 *per cent.*	p = 33·831	log. p = 1·5293132
4	25·497	1·4064846
5	20·497	1.3116680
6	17·162	1·2345630

No. LXIV.

A REVIEW OF

"AN ESSAY ON DEW

AND SEVERAL APPEARANCES CONNECTED WITH IT.

By William Charles Wells, M.D., F.R.S."

From the Quarterly Review for October, 1814.

The experiments, related in this Essay, have very clearly illustrated the nature and formation of dew, and very satisfactorily established the ingenious author's theory respecting it; a theory which, if not altogether so original as he supposes it, has certainly never been brought forwards in so striking and simple a form; nor indeed was it possible that it should be completed, at any time previous to the important discoveries, respecting the radiation of heat, which have been made within the last ten years; although, when it is understood that the properties of all bodies, with regard to cooling, are the exact counterparts of those which they exhibit in heating, the whole difficulty of the subject vanishes.

The sun's rays pass through the atmosphere, in the absence of clouds, with little immediate effect on its temperature; they strike on the earth, and the earth is much more heated by them than the air: in a clear night the reverse of this happens; the surface of the earth throws off heat by radiation more rapidly than the air, and when there are no clouds to intercept and reflect it, this surface is reduced to a temperature lower than that of the air in its neighbourhood: the difference is still more marked in light substances, in imperfect contact with the earth, and Dr. Wells has shown that, in such cases, it often actually amounts to 15 or 20 degrees.

It being once established that such a cause is sufficient for the production of a greater degree of cold at the surface of the earth than elsewhere, we may easily pursue its operation through all its consequences and combinations, which however are often very complicated; but in all instances it appears, that the production of cold must be previous to the deposition of moisture, and is not, as has sometimes been suspected, a consequence of that deposition, which, on the contrary, as Dr. Wells has very fully shown, like almost all other instances of condensation, is actually attended by the extrication of a certain portion of heat.

From calculations, founded on the experiments of Mr. Dalton, and other earlier observers, we infer that air, at the temperature of the freezing point, is capable of containing, when saturated with moisture, about $\frac{1}{200}$ of its weight of water in an invisible form; its capacity is doubled by raising its temperature 20°; again doubled by an elevation of 22°; then of 24°, 26°, 28°; and so on in succession. Thus at 52°, the air of a jar inverted in water will contain $\frac{1}{100}$ of its weight of moisture; at 74°, $\frac{1}{50}$; and at 98°, about $\frac{1}{25}$. The air of the atmosphere is generally in such a state as to require a depression of a few degrees for the deposition of a portion of the moisture which it contains: a glass of pump-water, or a pot of porter, from a cool cellar, becomes covered with a real dew in miniature, when brought into a room, by cooling the air in immediate contact with it. If humid but transparent air at 74° were cooled to 52°, it would deposit $\frac{1}{100}$ of its weight of water, and $\frac{1}{200}$ more if cooled again to 32°; and at all common temperatures, the depression of a single degree will occasion a deposition of a little more than $\frac{1}{30}$ of the whole moisture contained in the air. Hence it is obvious that the differences of temperature, observed by Dr. Wells, must be amply sufficient to account for the deposition of dew under the circumstances which are commonly observed to occasion its appearance.

Professor Leslie, in his late work on the Relations of Air to Heat and Moisture, has estimated the quantity of water capable of being contained in air at the freezing point, from his own experiments, as equal to $\frac{1}{160}$ of the weight of the air, and has

supposed this quantity to be always doubled by each successive addition of 27° of temperature; so that the moisture would amount to $\frac{1}{80}$ at 59°, and at 86° to $\frac{1}{40}$, instead of $\frac{1}{35}$, which would be the result of our mode of determination: his estimate is therefore a little greater than ours in one case, and a little less in another; but we are disposed to prefer our own mode of calculation, because it is founded on more general views of the subject, which are sufficiently supported by a variety of experiments of different kinds.

The theory, advanced by Dr. Wells, is a consequence so simple and obvious of the principles deduced from the discoveries of Mr. Leslie, and other observers, and now generally admitted, that it only requires to be distinctly stated, and clearly understood, in order to be considered as satisfactory.* At the same time it would scarcely be just to omit inserting some account of the various arguments and experiments by which our author has thought it right to enforce his doctrines; and in pursuing this detail, we shall find a number of miscellaneous facts and remarks, which are by no means unimportant.

It was observed by Aristotle, that dew appears only on clear and calm nights: when the weather is both cloudy and windy, it is scarcely ever deposited: and Dr. Wells has found, that whatever diminishes the exposure of any substance to the unclouded sky proportionally diminishes the quantity of dew that it receives; thus ten grains of wool, placed upon a horizontal board, acquired, in the course of a night, fourteen grains of moisture, while a similar quantity, attached to the lower surface of the board, gained only four grains. Light and detached substances also receive dew much more abundantly

* In the Annals of Philosophy for 1815, Dr. Wells, in a reply to some of the criticisms in this review, has denied the inference that his conclusions were a simple and obvious consequence of the discoveries referred to. Mr. Leslie's first essay was published in 1804, and in a work on heat and moisture published nine years afterwards, when noticing this subject and the remarkable speculations of Aristotle concerning it, the true theory was unknown to him as well as to other meteorologists, though it was suspected at least, if not fully manifest, to Dr. Young. It is a fact, which the history of philosophy everywhere makes known to us, that discoveries which, when made, seem to be easy consequences of principles previously established and understood, are reserved to the favoured few who can detect the gold which ordinary searchers overlook.—*Note by the Editor.*

than those which are more completely in contact with the solid earth: thus, while ten grains of wool, placed on a grassplat, gained sixteen grains in weight, another portion, placed on a gravel walk, gained only nine, and on the mould of a garden, eight: nor was dew ever deposited on the bare ground, however exposed. Polished metals seldom exhibit the appearance of dew on their surface, although pieces of metal and glass, exposed at equal temperatures to the steam of hot water, exhibited equal dispositions to attract it; so that nothing analogous to elective attraction can be supposed to take place in such cases. What is said of dew, is equally applicable to hoar frost, which, as Aristotle truly observed, is merely frozen dew.

The second step of Dr. Wells's investigation was to ascertain the thermometrical differences attending the phenomena. He once observed a thermometer, placed on the grass, 14° lower than another four feet above it; but the passage of a cloud often raised the temperature of the grass several degrees. The wool above the board was 7° colder than the same substance immediately below it. The surface of a gravel walk was 16½° warmer than the neighbouring grass, which was similarly exposed, although the earth an inch below the grass was even warmer than the air. A very important fact in meteorology was also ascertained by these experiments, that a thermometer, fully exposed to a clear sky, often represents the temperature of the neighbouring substances 2°, 3°, or 4° below the truth; and that in order to avoid this source of error, it is necessary to prevent the radiation of its heat into the empty space, by covering its bulb with gilt paper, if it is intended to ascertain the actual temperature either of the air, or of any other substance in contact with it; and an error of a contrary nature may also sometimes occur, when heat is radiated copiously by the surrounding bodies, even in the absence of the sun's direct rays. A plate of metal, lying on a plat of grass, was observed to be 10° warmer than the grass surrounding it. One of the substances which exhibited the greatest degree of comparative cold was swansdown, which was once found 15° colder than the air a few feet above it. Mr. Wilson of Glasgow had once

observed snow as much as 16° colder than the atmosphere; and to this difference 2° may be added, for the correction of the temperature of the air as indicated by the thermometer. Dr. Wells even thinks it probable that, in cold and exposed countries, substances near the surface of the earth may be 30° or 40° colder than the air at a considerable height in the atmosphere.

The temperature of wool, exposed to the sky in dewy nights, was always found to be depressed below that of the neighbouring air, before it began to acquire any additional weight, and this depression was again often diminished while the dew was deposited; so that 5° or 6° of cold seem to be frequently prevented in this manner. Hence it happens that the difference between the temperature of the surface of the earth and the air is less in summer than in winter, when there is less moisture to be deposited. A second caution of importance, in practical meteorology, relates to the use of the hygrometer; which, if fully exposed to the sky, may become much colder than the surrounding air, and thus exhibit a very erroneous indication, in consequence of the deposition of moisture, from air not previously saturated with it.

Theophrastus remarks, that the effects of cold are generally most hurtful in hollow places; and our author explains this phenomenon from the greater stillness of the air in confined situations, allowing the process of cooling to go on without interruption from the approach of fresh portions of air, which would afford heat both by direct communication, and in consequence of the deposition of moisture. That the air is not wholly incapable of emitting and receiving heat by radiation, as well as by direct communication, is proved by the heat of the atmosphere observable in the day time, during calm weather, in the middle of the largest oceans, while the water below it is considerably colder. Dew has sometimes been supposed to originate altogether from vapours rising out of the earth: thus a metal will often collect dew on its lower surface only, when it is of the same temperature with the air immediately surrounding it; but it is sufficiently obvious, from the experiments which have been related, that the most copious

source of dew is the moisture previously contained in the atmosphere. An inside shutter often favours the deposition of moisture on a window in the night time, by preventing the radiation of heat from the room. It will be easily understood, that the effects of a clear sky must sometimes be perceived in the human body, producing, by means of the uncompensated radiation of heat, a greater sense of cold, than could be expected from the temperature of the air as exhibited by the thermometer.

"I had often," says Dr. Wells, p. 120, "in the pride of half knowledge, smiled at the means frequently employed by gardeners, to protect tender plants from cold, as it appeared to me impossible that a thin mat, or any such flimsy substance, could prevent them from attaining the temperature of the atmosphere, by which alone I thought them liable to be injured. But, when I had learned, that bodies on the surface of the earth become, during a still and serene night, colder than the atmosphere, by radiating their heat to the heavens, I perceived immediately a just reason for the practice, which I had before deemed useless. Being desirous, however, of acquiring some precise information on this subject, I drove into the earth of a grassplat four slender sticks, in such a manner, as to make them rise six inches perpendicularly above the grass, and form the corners of a square, the sides of which were two feet long. Over the upper ends of these sticks were drawn lightly the four corners of a fine cambric handkerchief, rendered by long wear still thinner than it had been originally, and having here and there a slight rent. In this disposition of things, therefore, nothing existed to prevent the free passage of air from the exposed grass, to that which was sheltered by the handkerchief, except the four small sticks, and there was no substance to radiate heat downwards to the covered grass, except the handkerchief itself. The temperature of the grass, which was thus shielded from the sky, was upon many nights examined by me, and always found warmer than that of neighbouring grass, which was uncovered, if this was colder than the air. When the difference in temperature, between the air several feet above the ground and the unsheltered grass, did not exceed 5°, the sheltered grass was about as warm as the air; if that difference, however, exceeded 5°, the air was found to be somewhat warmer than the sheltered grass. Thus, upon one night, when fully exposed grass was 11° colder than the air, the latter was 3° warmer than the sheltered grass; and the same difference existed on another night, when the air was 14° warmer than the exposed grass. One reason for this difference

was, that the air, which passed from the exposed grass, by which it had been very much cooled, to the grass under the handkerchief, must have deprived the latter of part of its heat; another, that the handkerchief, from being made colder than the atmosphere by the radiation of its upper surface to the heavens, would remit less heat to the grass beneath than what it received from that substance. But still the sheltered grass, notwithstanding these drawbacks, was upon one night 8°, and upon another 11° warmer than grass fully exposed to the sky, which are differences sufficiently great, to explain the utility of a very slight shelter to plants, in averting or lessening injury from cold, on a still and serene night.

"In the next place, in order to learn whether any difference would arise from placing the sheltering substance at a much greater distance from the ground, I had four slender posts driven perpendicularly into the soil of a grass field, so as to be six feet eminent above the surface, and to form the angles of a square having sides eight feet in length. Over these was thrown an old ship flag of a very loose texture. Concerning the experiments carried on by means of this disposition of things, I shall only say, that they led to the conclusion, as far as the events of different nights could rightly be compared, that the higher shelter had the same efficacy with the lower, in preventing the occurrence of a cold upon the ground, in a clear night, greater than that of the atmosphere, provided the oblique aspect of the sky was equally excluded from the spots on which my thermometers were laid.

"On the other hand, a difference in temperature, of some magnitude, was always observed on still and serene nights, between bodies sheltered from the sky by substances touching them, and similar bodies, which were sheltered by a substance a little above them. I found, for example, upon one night, that the warmth of grass, sheltered by a cambric handkerchief raised a few inches in the air, was 3° greater than that of a neighbouring piece of grass which was sheltered by a similar handkerchief actually in contact with it. On another night, the difference between the temperatures of two portions of grass, shielded in the same manner as the above mentioned, from the influence of the sky, was 4°. Possibly, experience has long ago taught gardeners the superior advantage of defending tender vegetables from the cold of clear and calm nights, by means of substances not directly touching them; though I do not recollect ever having seen any contrivance for keeping mats or such like bodies at a distance from the plants, which they were meant to protect.

"Walls, I believe, as far as warmth is concerned, are regarded as useful during a cold night, to the plants which touch them, or are near them, only in two ways; first, by the mechanical shelter which they

afford against cold winds, and secondly, by giving out the heat which they had acquired during the day. It appearing to me, however, that on clear and calm nights, those on which plants frequently receive much injury from cold, walls must be beneficial in a third way, namely, by preventing, in part, the loss of heat which they would sustain from radiation if they were fully exposed to the sky; the following experiment was made for the purpose of determining the justness of this opinion.

"A cambric handkerchief was placed perpendicularly to a grassplat, by means of two upright sticks, at right angles to the course of the air, and a thermometer was laid upon the grass close to the lower edge of the handkerchief, on its windward side. A thermometer thus situated was several nights compared with another lying on the same grassplat, but on a part of it fully exposed to the sky. On two of these nights, the air being clear and calm, the grass close to the handkerchief was found to be 4° warmer than the fully exposed grass. On a third, the difference was 6°. An analogous fact is mentioned by Gersten, who says that a horizontal surface is more abundantly dewed, than one which is perpendicular to the ground."

Dr. Wells has been singularly fortunate in illustrating the formation of ice in warm climates, which he has shown to depend on the radiation of heat, and not, as had generally been supposed, on the refrigerating effect of evaporation. It is necessary, for the success of this process, that the air should be still, which is a circumstance unfavourable to evaporation; it is found to succeed best in dewy nights, when the quantity of evaporation must be inconsiderable; the straw on which the pans containing water are placed, must not be wet, in order that it may not communicate heat from the ground, and the pans must be porous for a similar reason. A cold of 14°, or more, is often required for the purpose, and Dr. Wells found that evaporation in still air, at a low temperature, did not produce a cold of above a degree or two. He succeeded in freezing water in this country without any evaporation, when the air a few feet above the ground was at 37° or even 39°; the temperature of grass fully exposed being at the same time 30°. In Mr. Williams's experiments, the straw, on which the pans stood, appeared warmer than the water, because it was much sheltered by them from the sky. Dr. Wells found that the bottom of an empty pan kept pace in cooling with the pans

of water, until the congelation took place; some moisture was deposited on it, which afterwards froze; and in another experiment, the water itself had gained some grains in weight, while part of it was frozen, in an atmosphere of 37°.

In reasoning respecting the heat transmitted by mists, Dr. Wells observes, that since the diminution of light, as ascertained by Leslie's photometer, is small, "it will readily *be granted* that the same state of the atmosphere will also give transit to radiant heat:" it must however have occurred to him on reflection, that the indications of Leslie's instrument depend immediately on radiant heat, and are only applied indirectly to light; so that there is no occasion for the analogy from which he has derived his argument.

It is not with a view of detracting from the merit of our author's laborious series of experiments, that we feel ourselves compelled to enter a protest against the total novelty of the opinions which they have so amply illustrated and confirmed. Dr. Wells appears, in his historical account of the doctrines relating to the nature and causes of dew, to have undertaken to afford us complete information respecting the sentiments not only of Aristotle and Theophrastus, but also of the "most distinguished" philosophers of modern times, p. 131: some of the works, however, of the persons whom he mentions, and some of the latest, have most unaccountably escaped his attention.

"Mr. Prévost of Geneva," says Dr. Wells, "in his work on Radiant Heat, has already in this way accounted for the effect of clouds in diminishing the cold of the atmosphere at night; but he seems not to have known, that they have a much greater effect of the same kind on the temperature of bodies upon the surface of the earth. My explanation of the latter operation of clouds is a direct consequence from the facts which I had observed respecting the prevention of cold on the ground from radiation, by the interposition of solid bodies between it and the heavens, and occurred to me in 1812. Mr. Prévost's work, indeed, was published in 1809, but I did not see it before the summer of 1813, when it was lent to me by his relation Dr. Marcet of London, who at the same time said, that he believed there was no other copy of it in Great Britain, except one, which had been sent by himself to Edinburgh."—p. 79.*

* This particular Essay was unknown to Dr. Wells, but he has cited a work of

Now we have at this moment before us a copy of Mr. Prévost's Recherches Physico-mécaniques sur la Chaleur, printed at Geneva in 1792; from which, for the sake of greater authenticity, we shall extract some passages in the original language.

"Sect. 24. *Phénomène.* La nuit, lorsque le ciel est serein, l'air est généralement plus froid près de la terre. Au printems et en automne, il gèle peu lorsque le ciel est couvert. Souvent enfin, par une nuit séreine, s'il vient à passer un nuage par le zénith de l'observateur, à l'instant il voit monter le thermomètre.

"Sect. 25. *Essai d'explication.* L'air même le plus dense, tel que celui de nos plaines, est perméable à la chaleur rayonnante; car c'est dans cet air qu'on observe celle-ci. L'air rare des régions supérieures de l'atmosphère est encore plus perméable; il est en quelque sorte transparent, ou plutôt *transcaloreux*. Mais l'eau ne l'est pas, ni la vapeur, vésiculaire. Les nuages sont opaques pour la chaleur comme pour la lumière. Ils absorbent l'une et l'autre, et ne la laissent passer que lentement.

"Ainsi la chaleur rayonnante de la terre traverse avec facilité l'atmosphère pure, mais elle est interceptée par les nuages. Ceux-ci font donc pour la terre une espéce de vêtement. Ils empèchent l'écoulement de sa chaleur rayonnante; et en la recevant vers leur partie inférieure, ils s'échauffent de ce côté-là, comme un habit s'échauffe du côté du corps, et par conséquent ils renvoient à la terre un peu plus de chaleur rayonnante que ne peut faire l'air transparent.

"La surface supérieure du nuage se refroidit, au contraire, par l'émission facile de sa chaleur dans un air raréfié. Mais le passage lent de la chaleur gênée, qui serpente de l'une à l'autre surface, ne peut rétablir l'équilibre incessamment rompu par la source inépuisable de chaleur du côté de la terre, et par le gouffre toujours ouvert où elle se précipite de l'autre.

"Tout nuage la nuit est donc exactement comparable à un vêtement très épais, qui recouvre un corps maintenu chaud par une cause interne et perpétuelle (tel qu'est, par exemple, le corps humain). La surface intérieure est chaude, la surface extérieure participe à la température froide de l'air ambiant. Et l'application du vêtement sur le corps y maintient la chaleur.

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the same author, published in 1809, in which the passages quoted in the text are reprinted: a fact which was unknown to Dr. Young. Dr. Wells contends, and we think successfully, that M. Prévost was ignorant of the important fact that in a clear night the surface of the earth becomes colder than the air above it.—*Note by the Editor.*

"On n'a pas lieu d'être surpris de la promptitude de l'effet, parceque tout le jeu de la chaleur rayonnante, allant et revenant de la terre au nuage et du nuage à la terre, s'exécute en un instant indivisible. D'ailleurs à l'instant où le nuage arrive au zénith, il arrive en quelque sorte tout préparé. Sa partie inférieure a déjà acquis une chaleur excédante. Déjà elle émet plus de chaleur rayonnante que pareille étendue d'air de la même région. C'est un lambeau de vêtement, qui passe d'une partie du corps à l'autre. Ainsi à l'instant même où ce vêtement chaud vient couvrir l'observateur, le thermomètre doit accuser sa présence.

"SECT. 142. Le phénomène météorologique, indiqué au Sect. 24, a été remarqué par M. Pictet, et consigné dans ses journaux d'observation. C'est ce qu'atteste l'extrait suivant, qu'il en a transcrit textuellement, et auquel il a joint une remarque importante. '*Janvier*, 1777. *Dans la nuit du* 4 *au* 5, *le thermomètre étoit à* —12 [5°] *à* 10 *heures du soir; le tems s'étant couvert ensuite, il n'étoit plus qu'à* —$10\frac{1}{4}$ [$8\frac{2}{9}$°] *à* 11 *h. du soir*. Je me rappelle distinctement, au sujet de cette note, (ajoute M. Pictet, en me la communiquant,) un fait que je ne trouve pas enregistré, c'est que le haussement de température dont il est question fut simultanée avec l'apparition d'un nuage assez voisin, mais peu étendu, aux environs du zénith.

"Un autre fait, observé par tous les agriculteurs, et relatif à l'influence prompte et presqu'immédiate des nuages sur le sol (indépendamment de leur effet pour intercepter les rayons solaires), est celui-ci: on sait que dans les circonstances les plus favorables d'ailleurs à l'apparition *de la rosée*, elle est *nulle*, ou presque nulle, si le ciel est *couvert;* et que les blanches gêlées, si redoutables au printems et en automne, n'ont pas lieu à *même température, si le temps est couvert.*

"*Tous les faits* mentionnés dans cette remarque de M. Pictet *s'expliquent* naturellement par les principes posés au Sect. 25, c'est à dire, *en considérant les nuages comme le vêtement du sol*, et en ayant égard à la chaleur rayonnante."

Nor were these doctrines by any means unknown in our own country; we find, for instance, in a Course of Lectures published in London seven years ago, that "when the weather has been clear, and a cloud passes over the place of observation, the thermometer frequently rises a degree or two almost instantaneously: this has been partly explained by considering the cloud as a vesture, preventing the escape of the heat which is always radiating from the earth, and reflecting it back to the surface."

It is true that the theory could only be completed by the application of Professor Leslie's discoveries to the circumstances of the phenomenon: but it is remarkable that this very application was made, in a case confessedly *similar*, by the author of the same work which we have last quoted.

"I once intended," says Dr. Wells, p. 105, "to add here an explanation of some very curious observations by Mr. Prévost of Besançon on dew, which were published first by himself, in the 44th number of the French Annals of Chemistry, and afterwards by Mr. Prévost of Geneva, in his Essay on Radiant Heat; but fearing to be very tedious, I have since given up the design. I will say, however, that, if to what is now generally known on the different modes in which heat is communicated from one body to another, be added the two following circumstances, that substances become colder than the air before they attract dew, and that bright metals, when exposed to a clear sky at night, become colder than the air much less readily than other bodies, the whole of the appearances observed by Mr. Prévost may be easily accounted for."

"It has been observed," says the author of the Course of Lectures published in 1807, "that a piece of metal, placed on glass, usually protects also the opposite side of the glass from the deposition of dew; and Mr. Benedict Prévost has shown, that, in general, whenever the metal is placed on the warmer side of the glass, the humidity is deposited more copiously either on itself, or on the glass near it," [as in the case of the shutter]; "that when it is on the colder side, it neither receives the humidity, nor permits its deposition on the glass; but that the addition of a second piece of glass over the metal destroys the effect, and a second piece of metal restores it. It appears that, from its properties with respect to *radiant heat*, the *metallic surface* produces these effects by preventing *ready communication* either of heat or *of cold* to the glass."*

* In page 474 of the second volume of Dr. Young's Lectures, there is an account, extracted from the Journals of the Royal Institution, of the principal facts contained in Benedict Prévost's Essay on Dew, with an observation preceding it, that they appeared to be capable of explanation by means of Mr. Leslie's discoveries. Dr. Wells was not aware, when his Essay was composed, that the publication of the second volume of the Lectures preceded the first, and he very naturally concluded therefore, that the author regarded his explanation not merely as a conjecture, but as a conjecture inapplicable to the whole of the facts which M. Prévost had observed. There can be no doubt, however, that the explanation in the text, which was written or revised subsequently to the former, embraced the basis of the true theory, and completely accounted for the most important circumstances which are recorded in these experiments.

The concluding paragraph of this review expresses the suspicion, which in later life became a conviction, that the merits and discoveries of its author were so insufficiently recognised and acknowledged because his writings were obscure and rarely understood: it was a fatal error to conceive that symbolical could be superseded

Had Dr. Wells been as solicitous to attend to the labours of his contemporaries as he has been very laudably anxious to recur to those of his predecessors, he might have said, not that the experiments of Mr. Prévost *might* "be easily accounted for" from the properties which he mentions, but that they actually *had* been explained in a similar manner by one of his own countrymen. There are, however, some modern philosophers, who, whether from their own fault, or from that of their hearers and readers, or from both, appear to be perpetually in the predicament of the celebrated prophetess of antiquity, who always told truth, but was seldom understood, and never believed: and the author of the Lectures in question has not unfrequently reminded us of the fruitless vaticinations of the ill-fated Cassandra.

by ordinary language in long and difficult investigations, and admirable as his Lectures are for the accuracy and precision of their statements of the principles of natural philosophy and of the principal results to which they lead, it is probable that no reader has ever had the ability or the patience thoroughly to appreciate them.—*Note by the Editor.*

No. LXV.

ON WEIGHTS AND MEASURES.*

From the Supplement to the Encyclopædia Britannica, vol. vi. p. 785.

The bill for ascertaining and establishing Uniformity of Weights and Measures, which passed the Imperial House of Commons in the Session of 1823, not having been carried through the House of Lords, the subject has less immediate interest at the present moment than there has been every reason to suppose, for a few years past, that it would by this time have acquired. But there is little doubt that the discussion will be revived either in the Upper or the Lower House of Parliament before many more months have elapsed; and it seems to be highly improbable that any insuperable difficulties should stand in the way of its ultimate success.† This presumption is founded on no hasty view of the subject in question, but upon a laborious and somewhat painful examination of the historical progress of the measures which have been taken respecting it, and especially of the laws of England respecting uniformity of practice in different parts of the country; a uniformity which, though generally esteemed by all governments a thing to be encouraged and enforced, has often proved to be no more subjected to the control of legislative enactment, than the introduction of a uniformity of language and a grammatical accuracy of speech would be found in every part of an extensive empire.

Augustus is said to have endeavoured in vain to force a new Latin word into the language of ancient Rome; the French, on

* A portion only of the original article is reprinted: the remainder chiefly consists of the Report of the Royal Commission appointed for the revision of weights and measures, in 1818, which was drawn up by Dr. Young.—*Note by the Editor.*

† This Act for securing uniformity in weights and measures was passed in 1824. —*Note by the Editor.*

the other hand, after all their labours to recommend a uniform system of measures, have ended in such a complication, that for the most simple purposes of practical mechanics and civil life, it is become usual to carry in the pocket a little ruler, in the form of a triangular prism, one of the sides containing the old established lines and inches of the royal foot, a second the millimètres, centimètres, and decimètres of the revolutionary school, and the third the new ultra-royal combination of the Jacobin measure with the royal division, the inches consisting each of the 36th part of a mètre, or the four millionth of a degree of the meridian of the earth. If such occurrences as these be calmly considered, they will make us more disposed to diminish than to increase the number of penal statutes intended to compel the inhabitants of the different parts of a country to study their own convenience conjointly with that of their neighbours, and to spare themselves the necessity of a few arithmetical operations in the course of every market day; and we shall feel that it is more incumbent on a wise government to endeavour to facilitate both the attainment of correct and uniform standards of legal existing measures of all kinds, and the ready understanding of all the provincial and local terms applied to measures, either regular or irregular, by the multiplication of glossaries and tables for the correct definition and comparison of such terms.

Measures have apparently always been derived, in the first instance, from some part of the human person; a foot, a pace, a fathom, the orgyia or stretch of the arms, a cubit, a palm, and a finger; these have probably all been used in the earlier states of society by each individual from the magnitude of his own person; and afterwards a standard measure has been established by authority from the real or supposed magnitude of the person of some king or hero, in order for the attainment of more perfect uniformity in practice; though it is said, that in some parts of the East the Arabs still measure the cubits of their cloth by the fore arm, with the addition of the breadth of the other hand, which serves to mark the end of the measure, as the thumb which was formerly added at the end of the yard by the English clothiers. It ought not, however, to be forgotten that any one of these terms possesses an advantage,

for popular use and for the convenience of future ages and of remote countries, which would be lost by the introduction of any more arbitrary measurement; thus, a hand's breadth, or a foot, is always sufficiently understood, without any definition, to enable us to form to ourselves a tolerably accurate picture of the magnitude intended to be described; and there is scarcely an instance of the caprice of denomination having ever extended so far as to make the measure called a foot in any country so small as half a natural foot, or so great as two feet of an ordinary person, and certainly not of its amounting to three ordinary feet; while a mètre, even to those who know that the word implies a measure, might as well have meant a mile, or an inch, or a quart, as a length somewhat greater than a yard.

The idea of accurately verifying the standard of a country by any other means than that of a comparison with some actually existing original, can scarcely have occurred, except in a very advanced period of the progress of civilization. It was indeed enacted, in the time of our Henry the Third, that an ounce should be the weight of 640 dry grains of wheat taken from the middle of the ear, that a pound should be twelve ounces, a gallon of wine eight pounds, and eight gallons of wine a bushel of London; but this seems rather a direction for making a single standard than a mode intended for the continual verification of the standard in case of any minute uncertainty. Again, in a statute of Henry the Seventh, a gallon of corn was mentioned as containing eight pounds of wheat; and this may, perhaps, serve to explain the origin of the two different gallons. But the substitution of an original standard, derived from an object of definite magnitude, exterior to the human person, seems to have been reserved for the days of the French Revolution, though it has since been adopted, in an improved form, by the introduction of a foot equal to $\frac{12}{38}$ of the pendulum vibrating seconds as a representative of the customary foot of the kingdom of Denmark. (*Journal R. I.* 1821, *Astr. Coll.* N. V.)

The Royal Society, under the presidency of Mr. Folkes, made some very accurate comparisons of the English, and French, and old Roman standards, which are recorded in the 'Philo-

No. LXVII.

ON THE HABITS OF SPIDERS:

ON PASSAGES IN ARISTOTLE AND ARISTOPHANES, AND AN ILLUSTRATION OF THE FABRICIAN SYSTEM.*

From the Gentleman's Magazine for Dec., 1792.

MR. URBAN, 5th 11mo.

> *Ἦ ῥαδίως φεύγων ἂν, ἀποφύγοι δίκην,*
> *Ὅστις δίοιδε τοὔντερον τῆς ἐμπίδος.*
>
> ARISTOPH. Nub.

HOWEVER ironically this remark was made, yet it is more true, and of more importance, than the witty comedian meant to imply. If I were convinced that a surgeon could skilfully "couch a dew-snail's eye," I could undoubtedly submit with absolute confidence to his performing that operation on mine: and, even in the instance of Strepsiades, I should be more willing to intrust with my cause a lawyer who had enlarged his mind by universal science, than one who had slavishly confined himself to the studies of his profession. For, the habit of accurate observation and nice distinction exercises and improves the faculties: *est etiam illa Platonis vera....vox, omnem doctrinam harum ingenuarum et humanarum artium uno quodam societatis vinculo contineri.* Cic. de Orat., III. This celebrated sentiment cannot be too strongly inculcated; it may serve as a general

* The earliest production of Dr. Young was a note on Gum Ladanum, with a verbal criticism on a passage in Longinus, inserted in the Monthly Review for 1791. The second, which appeared in the Gentleman's Magazine for April, 1792, contained observations on the appearances attending the conversion of cast into malleable iron, with a view to the removal of some of the objections to Dr. Crawford's Theory of Heat, which had been advanced by Dr. Beddoes in a paper published in the Philosophical Transactions for 1791. The third was the article which we have reprinted in the text. They all of them, and especially the last, serve to illustrate the singular combination of his studies, including natural history in all its departments, chemistry and classical literature: the Greek criticisms, which it contains, are sufficient proofs of the accurate knowledge which he had already acquired of that language.—*Note by the Editor.*

answer to the objections of those who consider pursuits of this nature as trifling and unimportant, because they cannot see their immediate application to the purposes of life.

From this apology I shall proceed to answer some inquiries on entomological subjects.

To Everard, *on Spiders*, p. 747.

"Io avea il dì cinque di Luglio fatto inchiudere un ragno femmina in un vaso di vetro serrato con carta;—non posi nel vaso cosa alcuna da poter nutricarsi;—la quale morì poi il dì trenta di Dicembre.—Altri ragnateli ancora e maschi, e femmine, feci rinchiudere ne' vasi di vetro; ma non trovai altro da osservare che la lunghezza della lor vita senz' alimento, essendo che alcuni presi a' quindici di Luglio camparono sino alla fine di Gennajo."—Redi, Esper. Op., t. I., p. 55, 57.

"I had placed, on the 5th of July, a female spider in a glass vessel closed with paper; I put nothing in the vessel on which it could feed; it died on the 30th of December. Other spiders also, both male and female, I enclosed in glass vessels; but I found nothing to observe except the length of their life without food; for, some of them, caught the 15th of July, held out till the end of January."

"Aranei nihil recondunt quod diu sine cibo vivere possunt; per hyemem verò ex toto abstinent, et ne victum quidem quærunt; ipsique ut plurimum per id tempus telis involuti conquiescunt, at non torpidi interim, sed æque agiles ac cum foras prodeunt venatum."—Lister, de Aran., p. 12.

"Spiders lay nothing by, because they can live long without meat. In the winter they neither eat nor seek for food; and throughout this season they mostly rest involved in webs, not torpid, but as active as when they go out to hunt."

Depending on these authorities, I kept last summer several spiders for the sake of breeding, without being very careful to supply them with flies; indeed they seldom condescended to make use of what I brought them; some of them lived one week, some two, but I think none exceeded three weeks. I have reason to doubt the general accuracy of Lister's account of the hyemation of Spiders.

I shall take this opportunity of attempting to vindicate Aristotle from an imputation which, I believe, is ill-founded. In the next page, Lister says—

"Quod autem id genus muscæ araneorum ova depascunt, inque ipsis eorum folliculis, ideo suum fœtum pariunt, in altero libro non uno sub titulo demonstravimus. Quæ quidem observatio male intellecta, vesparum ichneumonôn fabulæ, apud veteres adeo decantatæ, benè locum dare potuit. De quâ re ita Aristoteles, 'Vespæ ichneumones nuncupatæ, minores quàm cæteræ sunt, phalangia perimunt, occisaque ferunt in parietinas, aut aliquid tale foramine pervium; deinde illinunt luto, atque ex iis incubando suum procreant genus.'"

I allow that Gaza's translation is liable to the objection of Lister; but he appears to me to have mistaken the sense of his author. Aristotle's words are these: Καὶ πηλῷ καταχρίσαντες, ἐντίκτουσιν ἐνταῦθα, καὶ γίνονται ἐξ αὐτῶν οἱ σφῆκες οἱ ἰχνεύμονες. Hist. An., V. 20. Now ἐντίκτω is rendered by the Lexicons *ingenero;* and, if it admits of no other interpretation, Gaza is right. But in the preceding chapter we have, Αἱ δὲ κάνθαροι, ἣν κυλίνσι κόπρον, ἐν ταύτῃ φωλεύουσί τε τὸν χειμῶνα καὶ ἐντίκτουσι σκώληκας, ἐξ ὧν γίνονται κάνθαροι; parvosque vermiculos pariunt ex quibus ipsi procreantur, Gaza. "The *scarabæus pilularius* buries itself for the winter in the dung which it rolls along, and deposits in it the maggots from which the beetles are produced." This seems to be a sufficient authority for translating ἐντίκτουσι "lay their eggs;" and by this alteration we reconcile Aristotle's assertion with modern observations, except as to the literal meaning of the word καταχρίσαντες, *besmearing.* I will not be very positive on either of these passages, for I have to combat with the authority of the scholiast of Aristophanes on the curious piece of natural history introduced at the beginning of the Pax: Λέγεται δὲ ὁ κάνθαρος εἰς ὄνθον ἀποσπερματίζειν—θῆλυς γὰρ κάνθαρος οὐ γίνεται. "Dicitur autem scarabæus pilularius in fimum semen ejicere—for there are no females of this species." It may also be objected, that this translation of the passage concerning the *scarabæus pilularius* supposes it viviparous; but it appears, from c. 20, that Aristotle imagined some insects to be viviparous.

In reply to J. O., p. 920, the Grub is the *larva* of the *scarabæus melolontha*, or cockchafer. In some counties, the insect in its perfect state is called Grub, in others Oakub,

probably a corruption from Oak grub. For five years it is in motion, and four weeks at rest under-ground, Fabr. Phil. Ent. p. 159, and probably about a month an inhabitant of the air in its full perfection. In its first state, it incommodes the human race; and, in its last, it is persecuted in return. The custom of flying beetles was as common among children two thousand years ago as at present, though the ancient mode was more humane than the modern: Μὴ νῦν περὶ σαυτὸν εἶλε τὴν γνώμην ἀεὶ, 'Αλλ' ἀποχάλα τὴν φροντίδ' ἐς τὸν ἀέρα, Λινόδετον ὥσπερ μηλολόνθην τοῦ ποδός. Aristoph. Nub. "Now, do not button up thy mind perpetually about thyself, but let loose thy thoughts into the air like a beetle tied by the foot with a thread." But the *melolontha* of the Greeks was certainly not our *melolontha;* for, Aristotle tells us that the *larva* was found in dung, V. 19. It was a golden beetle: pretty clearly the *scarabæus auratus*, a much more elegant insect than the cockchafer.

I can assure L. E. that crickets have mouths: an insect of the same genus nearly gnawed through the skin of my finger a few months since; and had I permitted it to proceed, would soon have made a wound. I have applied to a baker, who has undertaken to procure me a cricket; and I intend shortly to send a drawing and description of its complicated apparatus for feeding.

In answer to T. W., p. 639. A relation of mine attempted last summer to pick out a harvest-bug, and show it me; a little red substance was indeed more than once exhibited, but it was so much injured in the operation, that I could not determine whether it was the insect or a small portion of dried blood. There is, however, little doubt that it is an *acarus*, very similar to the *acarus siro*, or mite, and still more nearly allied to the *acarus scabiei*, or itch-animal. I believe it is not named either by Linné, or by his editor, Gmelin. I was, perhaps, the more cautious in examining the red substance, from a circumstance which happened about the same time. I had been almost persuaded to believe that I saw a minute worm extracted from a decayed tooth; while, in fact, the beak of a seed of henbane was the whole curiosity. This story,

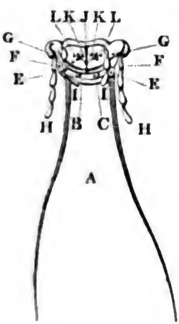
however, is as old as Avicenna. A lady of 80 could clearly discover in my microscope that its head was precisely like that of a dog. As my eyes were not quite so acute, I suspended my judgment till I could procure more of the seed; but, in the mean time, that nothing might be lost, I described and arranged what I saw under the name of *sinodon odontalgicus.* When I was informed of the true state of the case, I recollected the precept of Epicharmus: Νῆφε καὶ μέμνασ' ἀπιστεῖν, ἄρθρα ταῦτα τῶν φρενῶν. Cic. ad Att. 1. 19; or, as his brother Quintus expresses it, "nervos atque artus esse sapientiæ nil temere credere," de Pet. Cons. *that it is the joints and sinews of wisdom to believe nothing rashly.*

THOMAS YOUNG.

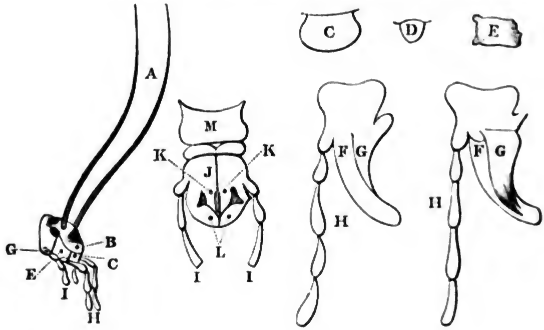
MR. URBAN, Little Queen Street, Westminster, 9th 11mo.

HAVING procured a cricket, I now send a drawing and description of its mouth. It was in the state of a *pupa*; but this circumstance seldom makes any difference in the mouth, where the mode of life is the same in all states.

GRYLLUS DOMESTICUS, *Linn.* ACHETA DOMESTICA, *Fabr.* *magnified.*



A, antennæ.
B, facies.
C, clypeus, seu labium superius.
D, palatum.
E, mandibula.
F, galea.
G, maxilla.
H, palpi anteriores.
I, palpi posteriores.
J, labium, seu labium inferius.
K, laciniæ interiores;
L, exteriores.
M, gula.



A. *Antennæ* black-brown, bristled :* segments very numerous, very short.

B. *Face* luteous and brown-black.

C. *Upper Lip* luteous, oval, membranous-horny, covering the tip of the mandibles and the upper part of the mouth.

D. *Palate* light brown, prominent, half-egged within the mandibles.

E. *Mandibles* luteous at the tip, blackish, incurved somewhat obliquely, lopped, toothed, opening transversely, constituting the lower part of the cheeks inclosing the upper part of the mouth; the tip covered by the upper lip.

F. *Gums* whitish, fleshy, cylindric, obtuse, incurved, somewhat longer than the jaws which they cover and with which they open and close the lower part of the mouth.

G. *Jaws* whitish, horny-fleshy, oblong, pointed, incurved at the tip, blackish, horny, cloven: divisions acute, back bearing the fore palps.

H. *Fore palps* whitish, fleshy, longer, fixed to the back of the jaws, five-jointed: segments nearly inverse-awled: first very short; second somewhat longer; the remaining three nearly equal.

I. *Hind palps* whitish, fleshy, shorter, fixed to the lip at the

* Tapered, length many times exceeding the breadth.

sides of its base, three-jointed; segments nearly inverse-awled; first, very short; second and third, nearly equal.

J. *Lip* or *lower Lip* whitish, fleshy-membranous, flattish, rounded, inclosing the mouth beneath: base bearing the hind palps; tip four-cleft:

K. Interior divisions smaller, awled.

L. Exterior larger, clubbed, bent inwards, concealing the tip of the interior.

M. *Gullet* whitish.

Many of the parts are slightly hairy.

The presence of gums entitles this genus to a place in Fabricius's second class, ULONATA, which comprehends the forficula, mantis, blatta, and gryllus of Linné. Unequal threadform palps, a four-cleft lip, and bristled antennæ, distinguish the *acheta* of Fabricius. So far then is this insect from being without a mouth, that it is furnished, like the greater number of other insects, with four projecting instruments for examining, and a double apparatus for chewing its food, besides the other appendages, of which it exhibits very good specimens, illustrating most of the terms employed in describing the mouths of coleopterous, and this division of hemipterous insects.

THOMAS YOUNG.

P.S. Since I wrote my last letter, Dr. Shaw has informed me that he has given a figure and description of the harvest bug in his admirable Miscellany, and that he has named it *acarus autumnalis*.

T. Y.

BIOGRAPHIES OF MEN OF SCIENCE.

No. LXVIII.

LIFE OF CAVENDISH.*

HENRY CAVENDISH, a great and justly celebrated Chemist, Natural Philosopher, and Astronomer; son of Lord Charles Cavendish, and grandson of William, second Duke of Devonshire; born the 10th of October, 1731, at Nice, where his mother, Lady Anne Grey, daughter of Henry, Duke of Kent, had gone, though ineffectually, for the recovery of her health.

Of a man, whose rank, among the benefactors of science and of mankind, is so elevated as that of Mr. Cavendish, we are anxious to learn all the details both of intellectual cultivation and of moral character that the labours of a biographer can discover and record. Little, however, is known respecting his earliest education: he was for some time at Newcombe's school, an establishment of considerable reputation at Hackney; and he afterwards went to Cambridge: but it is probable that he acquired his taste for experimental investigation in great measure from his father, who was in the habit of amusing himself with meteorological observations and apparatus, and to whom we are indebted for a very accurate determination of the depression of mercury in barometrical tubes, which has been made the basis of some of the most refined investigations of modern times.† "It has been observed," says M. Cuvier, "that

* This is the first of a series of lives of men of science, selected from many others, which were written by Dr. Young for the Supplement to the Encyclopædia Britannica. A very remarkable Life of Porson, as well as of one or two other men of letters, will be given in the following volume.—*Note by the Editor.*

† See the article Cohesion, vol. i. No. XIX.

more persons of rank enter seriously into science and literature in Great Britain than in other countries: and this circumstance may naturally be explained from the constitution of the British Government, which renders it impossible for birth and fortune alone to attain to distinction in the state, without high cultivation of the mind; so that amidst the universal diffusion of solid learning, which is thus rendered indispensable, some individuals are always found who are more disposed to occupy themselves in the pursuit of the eternal truths of nature, and in the contemplation of the finished productions of talent and genius, than in the transitory interests of the politics of the day." Mr. Cavendish was neither influenced by the ordinary ambition of becoming a distinguished statesman, nor by a taste for expensive luxuries or sensual gratifications: so that, enjoying a moderate competence during his father's life, and being elevated by his birth above all danger of being despised for want of greater affluence, he felt himself exempted from the necessity of applying to any professional studies, of courting the approbation of the public either by the parade of literature or by the habits of conviviality, or of ingratiating himself with mixed society by the display of superficial accomplishments. It is difficult to refrain from imagining that his mind had received some slight impression from the habitual recurrence to the motto of his family: the words *cavendo tutus* must have occurred perpetually to his eye; and all the operations of his intellectual powers exhibit a degree of *caution* almost unparalleled in the annals of science, for there is scarcely a single instance in which he had occasion to retrace his steps or to recal his opinions. In 1760 he became a Fellow of the Royal Society, and continued for almost fifty years to contribute to the *Philosophical Transactions* some of the most interesting and important papers that have ever appeared in that collection, expressed in language which affords a model of concise simplicity and unaffected modesty, and exhibiting a precision of experimental demonstration commensurate to the judicious selection of the methods of research and to the accuracy of the argumentative induction; and which have been considered, by some of the most enlightened historians, as having been no less instrumental in pro-

moting the further progress of chemical discovery, by banishing the vague manner of observing and reasoning that had too long prevailed, than by immediately extending the bounds of human knowledge with respect to the very important facts which are first made public in these communications.

1. *Three Papers containing Experiments on Factitious Air.* (*Phil. Trans.* 1766, p. 141.) It had been observed by Boyle, that some kinds of air were unfit for respiration; and Hooke and Mayow had looked still further forwards into futurity with prophetic glances, which seem to have been soon lost and forgotten by the inattention or want of candour of their successors. Hales had made many experiments on gases, but without sufficiently distinguishing their different kinds, or even being fully aware that fixed air was essentially different from the common atmosphere. Sir James Lowther, in 1733, had sent to the Royal Society some bladders filled with coal-damp, which remained inflammable for many weeks—little imagining the extent of the advantages which were one day to result to his posterity from the labours of that society by the prevention of the fatal mischiefs which this substance so frequently occasioned. Dr. Seip had soon after suggested that the gas which stagnated in some caverns near Pyrmont was the cause of the briskness of the water; Dr. Brownrigg of Whitehaven had confirmed this opinion by experiments in 1741; and Dr. Black, in 1755, had explained the operation of this fluid in rendering the earths and alkalis mild. Such was the state of pneumatic chemistry when Mr. Cavendish began these experimental researches. He first describes the apparatus now commonly used in processes of this kind, a part of which had been before employed by Hales and others, but which he had rendered far more perfect by the occasional employment of mercury. He next relates the experiments by which he found the specific gravity of inflammable air to be about $\frac{1}{11}$ of that of common air, whether it was produced from zinc or otherwise: first weighing a bladder filled with a known bulk of the gas, and then in a state of collapse; and also examining the loss of weight during the solution of zinc in an acid, having taken care to absorb all the superfluous moisture of the gas by means of dry potass. He also

observed that the gas obtained during the solution of copper in muriatic acid was rapidly absorbed by water, but he did not inquire further into its nature. The second paper relates to fixed air, which was found to undergo no alteration in its elasticity when kept a year over mercury; to be absorbed by an equal bulk of water or of olive-oil, and by less than half its bulk of spirit of wine; to exceed the atmospheric air in specific gravity by more than one-half, and to render this fluid unfit for supporting combustion even when added to it in the proportion of 1 to 9 only. Mr. Cavendish ascertained the quantity of this gas contained in marble and in the alkalies, but his numbers fell somewhat short of those which have been determined by later experiments; he also observed the solubility of the supercarbonate of magnesia. In the third part, the air produced by fermentation and putrefaction is examined. Macbride had shown that a part of it was fixed air; and our author finds that sugar and water, thrown into fermentation by yeast, emit this gas without altering the quantity or quality of the common air previously contained in the vessel, which retains its power of exploding with hydrogen, exactly like common air: he also shows that the gas thus emitted is identical with the fixed air obtained from marble; and that the inflammable air, extricated during putrefaction, resembles that which is procured from zinc, although it appeared to be a little heavier.

2. *Experiments on Rathbone Place Water.* (*Phil. Trans.* 1767, p. 92.) In this paper Mr. Cavendish shows the solubility of the supercarbonate of lime, which is found in several waters about London, and is decomposed by the process of boiling, the simple carbonate being deposited in the form of a crust: the addition of pure lime-water also causes a precipitation of a greater quantity of lime than it contains. These conclusions are confirmed by synthetical experiments, in which the supercarbonate is formed and remains in solution.

3. *An Attempt to explain some of the principal Phenomena of Electricity by means of an Elastic Fluid.* (*Phil. Trans.* 1771, p. 584.) Our author's theory of electricity agrees with that which had been published a few years before by Æpinus, but he has entered more minutely into the details of calculation,

showing the manner in which the supposed fluid must be distributed in a variety of cases, and explaining the phenomena of electrified and charged substances as they are actually observed. There is some degree of unnecessary complication from the great generality of the determinations: the law of electric attraction and repulsion not having been at that time fully ascertained, although Mr. Cavendish inclines to the true supposition, of forces varying inversely as the square of the distance: this deficiency he proposes to supply by future experiments, and leaves it to more skilful mathematicians to render some other parts of the theory still more complete. He probably found that the necessity of the experiments, which he intended to pursue, was afterwards superseded by those of Lord Stanhope and M. Coulomb; but he had carried the mathematical investigation somewhat further at a later period of his life, though he did not publish his papers:* an omission, however, which is the less to be regretted, as M. Poisson, assisted by all the improvements of modern analysis, has lately treated the same subject in a very masterly manner. The acknowledged imperfections, in some parts of Mr. Cavendish's demonstrative reasoning, have served to display the strength of a judgment and sagacity still more admirable than the plodding labours of an automatical calculator. One of the corollaries seems at first sight to lead to a mode of distinguishing positive from negative electricity, which is not justified by experiment; but the fallacy appears to be referable to the very comprehensive character of the author's hypothesis, which requires some little modification to accommodate it to the actual circumstances of the electric fluid, as it must be supposed to exist in nature.

4. *A Report of the Committee appointed by the Royal Society, to consider of a Method for securing the Powder Magazine at Purfleet. (Phil. Trans.* 1773, p. 42. *Additional Letter*, p. 66.) Mr. Cavendish, and most of his colleagues on the committee, recommended the adoption of pointed conductors. Mr. Wilson protested, and preferred blunt conductors; but the committee persisted in their opinion. Later experiments, however, have shown that the point in dispute between them was of little moment.

* It is generally understood that Sir William Snow Harris, the eminent electrician, is engaged in the publication of these important papers.—*Note by the Editor.*

5. *An Account of some Attempts to imitate the Effects of the Torpedo by Electricity.* (*Phil. Trans.* 1776, p. 196.) The peculiarity of these effects is shown to depend in some measure on the proportional conducting powers of the substances concerned, and on the quantity of electricity, as distinguished from its intensity. Iron is found to conduct 400 million times as well as pure water, and sea water 720 times as well; and the path chosen by the electric fluid, depending on the nature of all the substances within its reach, an animal, not immediately situated in the circuit, will often be affected on account of the facility with which animal substances in general conduct the fluid. The shock of a torpedo, producing a strong sensation, but incapable of being conveyed by a chain, was imitated by the effect of a weak charge of a very large battery: and an artificial torpedo of wood being made a part of the circuit, the shock diffused itself very perceptibly through the water in which it was placed; but the experiment succeeded better when the instrument was made of wet leather, which conducts rather better than wood, the battery being more highly charged in proportion to the increase of conducting power.

6. *An Account of the Meteorological Instruments used at the Royal Society's House.* (*Phil. Trans.* 1776, p. 375.) Of the thermometers it is observed, that they are adjusted by surrounding the tubes with wet cloths or with steam, and barely immersing the bulbs in the water, since a variation of two or three degrees will often occur if these precautions are neglected. For the correction of the heights of barometers we have Lord Charles Cavendish's table of the depression arising from capillary action. The variation-compass was found to exhibit a deviation from the meridian 15′ greater in the house of the Royal Society than in an open garden in Marlborough Street; there was also a mean error of about 7′ in the indications of the dipping-needle, but it was difficult to ascertain the dip without being liable to an irregularity, which often amounted to twice as much.

7. *Report of the Committee appointed to consider of the best Method of adjusting Thermometers.* (*Phil. Trans.* 1777, p. 816.) This paper is signed by Mr. Cavendish and six other members,

but it is principally a continuation of the preceding. It contains very accurate rules for the determination of the boiling point, and tables for the correction of unavoidable deviations from them: establishing 29.8 inches as the proper height of the barometer for making the experiment, if only steam be employed, and 29.5 if the ball be dipped in the water; but with all precautions, occasional variations of half a degree were found in the results.

8. *An Account of a New Eudiometer.* (*Phil. Trans.* 1783, p. 106.) Mr. Cavendish was aware of the great difference in the results of eudiometrical experiments with nitrous gas, or nitric oxyd, according to the different modes of mixing the elastic fluids; and he justly attributes them to the different degrees of oxygenization of the acid that is formed. But he found that when the method employed was the same, the results were perfectly uniform; and he ascertained in this manner that there was no sensible difference in the constituent parts of the atmosphere under circumstances the most dissimilar: the air of London, with all its fires burning in the winter, appearing equally pure with the freshest breezes of the country. He also observed the utility of the sulphurets of potass and of iron for procuring phlogisticated air; but he does not seem to have employed them as tests of the quantity of this gas contained in a given mixture.

9. *Observations on Mr. Hutchins's Experiments for determining the degree of Cold at which Quicksilver freezes.* (*Phil. Trans.* 1783, p. 303.) In experiments of this kind, many precautions are necessary, principally on account of the contraction of the metal at the time of its congelation, which was found to amount to about $\frac{1}{23}$ of its bulk; and the results which had been obtained were also found to require some corrections for the errors of the scales, which reduced the degree of cold observed to 39° below the zero of Fahrenheit, or 71° below the freezing point, answering to —39.4° of the centesimal scale. In speaking of the evolution of heat during congelation, he calls it "generated" by the substances, and observes, in a note, that Dr. Black's hypothesis of capacities depends "on the supposition that the heat of bodies is owing to their containing more or less of a

substance called the matter of heat; and as" he thinks "Sir Isaac Newton's opinion, that heat consists in the internal motion of the particles of bodies, much the most probable," he chooses "to use the expression heat is generated," in order to avoid the appearance of adopting the more modern hypothesis; and this persuasion, of the non-existence of elementary heat, he repeats in his next paper. It is remarkable that one of the first of Sir Humphry Davy's objects, at the very beginning of his singularly brilliant career of refined investigation and fortunate discovery, was the confirmation of this almost forgotten opinion of Mr. Cavendish; and for this purpose he devised the very ingenious experiment of melting two pieces of ice by their mutual friction in a room below the freezing temperature, which is certainly incompatible with the common doctrine of caloric, unless we admit that caloric could have existed in the neighbouring bodies in the form of cold, or of something else that could be converted into caloric by the operation; and this transmutation would still be nearly synonymous with generation, in the sense here intended. However this may be, it is certain that, notwithstanding all the experiments of Count Rumford, Dr. Haldalt, and others, Sir Humphry has been less successful in persuading his contemporaries of the truth of Mr. Cavendish's doctrine of heat, than in establishing the probability of his opinions respecting the muriatic acid.

10. *Experiments on Air.* (*Phil. Trans* 1784, p. 119.) This paper contains an account of two of the greatest discoveries in chemistry that have ever yet been made public—the composition of water, and that of the nitric acid. The author first establishes the radical difference of hydrogen from nitrogen or azote; he then proceeds to relate his experiments on the combustion of hydrogen with oxygen, which had partly been suggested by a cursory observation of Mr. Warltire, a Lecturer on Natural Philosophy, and which prove that pure water is the result of the process, provided that no nitrogen be present.*

* M. Arago, in his Éloge of Watt, attempted to transfer to that philosopher the merit of this great discovery, and thus gave rise to a vehement controversy, which has been finally and conclusively settled in favour of Cavendish by Dr. Wilson, of Edinburgh. See his Life of Cavendish, 1851.—*Note by the Editor.*

These experiments were first made in 1781, and were then mentioned to Dr. Priestley; and when they were first communicated to Lavoisier, he found some difficulty in believing them to be accurate. The second series of experiments demonstrates that when phlogisticated air, or nitrogen, is present in the process, some nitric acid is produced; and that this acid may be obtained from atmospheric air by the repeated operation of the electrical spark.

It has been supposed by one of Mr. Cavendish's biographers, that if Mr. Kirwan, instead of opposing, had adopted his chemical opinions, "he would never have been obliged to yield to his French antagonists, and the antiphlogistic theory would never have gained ground." But in this supposition there seems to be a little of national prejudice. Mr. Cavendish by no means dissented from the whole of the antiphlogistic theory; and in this paper he has quoted Lavoisier and Scheele in terms of approbation, as having suggested the opinion "that dephlogisticated and phlogisticated air are quite distinct substances, and not differing only in their degree of phlogistication, and that common air is a mixture of the two." He afterwards mentions several memoirs of Lavoisier in which phlogiston is entirely discarded; and says that "not only the foregoing experiments, but most other phenomena of nature, seem explicable as well, or nearly as well, upon this as upon the commonly believed principle of phlogiston;" and after stating a slight conjectural objection, derived from the chemical constitution of vegetables, he proceeds finally to observe, that "Lavoisier endeavours to prove that dephlogisticated air is the acidifying principle:" this is no more than saying, that acids lose their acidity by uniting to phlogiston, which, with regard to the nitrous, vitriolic, phosphoric, and arsenical acids, is certainly true, and probably with regard to the acid of sugar; "but as to the marine acid, and acid of tartar, it does not appear that they are capable of losing their acidity by any union with phlogiston" and the acids of sugar and tartar become even less acid by a further dephlogistication. It is obvious that this argument amounts only to an exception, and not to a total denial of the truth of the theory: M. Cuvier has even asserted

that the antiphlogistic theory derived its first origin from one great discovery of Mr. Cavendish, that of the nature of hydrogen gas, and owed its complete establishment to another, that of the composition of water: but it would be unjust to deny to Lavoisier the merit of considerable originality in his doctrines respecting the combinations of oxygen; and however he may have been partly anticipated by Hooke and Mayow, it was certainly from him that the modern English chemists immediately derived the true knowledge of the constitution of the atmosphere, which they did not admit without some hesitation, but which they did ultimately admit when they found the evidence irresistible. On the other hand, it has been sufficiently established, since Mr. Cavendish's death, by the enlightened researches of the most original of all chemists, that Lavoisier had carried his generalization too far; and it must ever be remembered, to the honour of Mr. Cavendish, and to the credit of this country, that we had not all been seduced, by the dazzling semblance of universal laws, to admit facts as demonstrated which were only made plausible by a slight and imperfect analogy.

11. *Answer to Mr. Kirwan's Remarks upon the Experiments on Air.* (*Phil. Trans.* 1784, p. 170.) Mr. Kirwan, relying on the results of some inaccurate experiments, had objected to those conclusions which form the principal basis of the antiphlogistic theory. Mr. Cavendish repeated such of these experiments as seemed to be the most ambiguous, and repelled the objections; showing, in particular, that when fixed air was derived from the combustion of iron, it was only to be referred to the plumbago, shown by Bergmann to exist in it, which was well known to be capable, in common with other carbonaceous substances, of affording fixed air.

12. *Experiments on Air.* (*Phil. Trans.* 1785, p. 372.) The discovery of the composition of the nitric acid is here further established; and it is shown that the whole, or very nearly the whole of the irrespirable part of the atmosphere is convertible into this acid, when mixed with oxygen, and subjected to the operation of the electric spark: the fixed air, sometimes ob-

tained during the process, being wholly dependent on the presence of some organic substances.

13. *An Account of Experiments made by Mr. John Macnab, at Henley House, Hudson's Bay, relating to Freezing Mixtures.* (*Phil. Trans.* 1786. p. 241.) From these experiments Mr. Cavendish infers the existence of two distinct species of congelation in mixed liquids, which he calls the Aqueous and Spirituous Congelations, and of several alternations of easy and difficult congelation when the strength is varied, both in the case of the mineral acids and of spirit of wine. The greatest degree of cold obtained was $-78\frac{1}{2}^\circ$.

14. *An Account of Experiments made by Mr. John Macnab, at Albany Fort, Hudson's Bay.* (*Phil. Trans.* 1788, p. 166.) The points of easy congelation are still further investigated, and illustrated by comparison with Mr. Keir's experiments on the sulphuric acid. It was found that the nitric acid was only liable to the aqueous congelation, when it was strong enough to dissolve $\frac{1}{4}$th of its weight of marble; and that it had a point of easy congelation, when it was capable of dissolving $\frac{418}{1000}$, the frozen part exhibiting, in other cases, a tendency to approach to this standard. Mr. Keir had found that sulphuric acid, of the specific gravity 1.78, froze at 46°, and that it had another maximum when it was very highly concentrated.

15. *On the Conversion of a Mixture of Dephlogisticated and Phlogisticated Air into Nitric Acid, by the Electric Shock.* (*Phil. Trans.* 1788, p. 261.) Some difficulties having occurred to the Continental chemists in the repetition of this experiment, it was exhibited with perfect success, by Mr. Gilpin, to a number of witnesses. This was an instance of condescension, which could scarcely have been expected from the complete conviction, which the author of the discovery must have felt, of his own accuracy, and of the necessity of the establishment of his discovery, when time should have been afforded for its examination.

16. *On the Height of the Luminous Arch, which was seen on Feb.* 23, 1784. (*Phil. Trans.* 1790, p. 101.) Mr. Cavendish conjectures that the appearance of such arches depends on a

diffused light, resembling the aurora borealis, spread into a flattened space, contained between two planes nearly vertical, and only visible in the direction of its breadth: so that they are never seen at places far remote from the direction of the surface; and hence it is difficult to procure observations sufficiently accurate for determining their height, upon so short a base: but in the present instance there is reason to believe that the height must have been between 52 and 71 miles.

17. *On the Civil Year of the Hindoos, and its Divisions, with an Account of three Almanacs belonging to Charles Wilkins, Esq.* (*Phil. Trans.* 1792, p. 383.) The subject of this paper is more intricate than generally interesting; but it may serve as a specimen of the diligence which the author employed in the investigation of every point more or less immediately connected with his favourite objects. The month of the Hindoos is lunar in its duration, but solar in its commencement; and its periods are extremely complicated, and often different for different geographical situations: the day is divided and subdivided sexagesimally. The date of the year, in the epoch of the Kalee Yug, expresses the ordinal number of years elapsed, as it is usual with our astronomers to reckon their days: so that the year 100 would be the beginning of the second century, and not the 100th year, or the end of the first century, as in the European calendar: in the same manner as, in astronomical language, 1817 December 31d. 18h. means six o'clock in the morning of the 1st of January 1818.

18. *Experiments to determine the Density of the Earth.* (*Phil. Trans.* 1798, p. 469.) The apparatus, with which this highly important investigation was conducted, had been invented and constructed many years before by the Reverend John Michell, who did not live to perform the experiments for which he intended it. Mr. Cavendish, however, by the accuracy and perseverance with which he carried on a course of observations of so delicate a nature, as well as by the skill and judgment with which he obviated the many unforeseen difficulties that occurred in its progress, and determined the corrections of various kinds which it was necessary to apply to the results, has deserved no less gratitude from the cultivators of astro-

nomy and geography, than if the idea had originally been his own. The method employed was to suspend, by a vertical wire, a horizontal bar, having a leaden ball at each end; to determine the magnitude of the force of torsion by the time occupied in the lateral vibrations of the bar; and to measure the extent of the change produced in its situation by the attraction of two large masses of lead, placed on opposite sides of the case containing the apparatus, so that this attraction might be compared with the weight of the balls, or, in other words, with the attraction of the earth. In this manner the mean density of the earth was found to be 5½ times as great as that of water; and although this is considerably more than had been inferred from Dr. Maskelyne's observations on the attraction of Shehallion, yet the experiments agree so well with each other, that we can scarcely suppose any material error to have affected them. Mr. Michell's apparatus resembled that which M. Coulomb had employed in his experiments on magnetism, but he appears to have invented it before the publication of M. Coulomb's Memoirs.

19. *On an Improved Method of Dividing Astronomical Instruments.* (*Phil. Trans.* 1809, p. 221.) The merits of this improvement have not been very highly appreciated by those who are in the habit of executing the divisions of circular arcs. It consists in a mode of employing a microscope, with its cross wires, as a substitute for one of the points of a beam compass, while another point draws a faint line on the face of the instrument in the usual manner. The Duke de Chaulnes had before used microscopical sights for dividing circles; but his method more nearly resembled that which has been brought forwards in an improved form by Captain Kater; and Mr. Cavendish, by using a single microscope only, seems to have sacrificed some advantages which the other methods appear to possess: but none of them has been very fairly tried; and our artists have hitherto continued to adhere to the modes which they had previously adopted, and which it would perhaps have been difficult for them to abandon, even if they had been convinced of the advantages to be gained by some partial improvements.

Such were the diversified labours of a philosopher, who pos-

sessed a clearness of comprehension and an acuteness of reasoning which had been the lot of very few of his predecessors since the days of Newton. Maclaurin and Waring, perhaps also Stirling and Landen, were incomparably greater mathematicians; but none of them attempted to employ their powers of investigation in the pursuit of physical discovery: Euler and Lagrange, on the Continent, had carried the improvements of analytical reasoning to an unparalleled extent, and they both, as well as Daniel Bernoulli and D'Alembert, applied these powers with marked success to the solution of a great variety of problems in mechanics and in astronomy; but they made no experimental discoveries of importance: and the splendid career of chemical investigation, which has since been pursued with a degree of success so unprecedented in history, may be said to have been first laid open to mankind by the labours of Mr. Cavendish; although the further discoveries of Priestley, Scheele, and Lavoisier, soon furnished, in rapid succession, a superstructure commensurate to the extent of the foundations so happily laid. "Whatever the sciences revealed to Mr. Cavendish," says Cuvier, "appeared always to exhibit something of the sublime and the marvellous; he weighed the earth; he rendered the air navigable; he deprived water of the quality of an element;" and he denied to fire the character of a substance. "The clearness of the evidence on which he established his discoveries, so new and so unexpected as they were, is still more astonishing than the facts themselves which he detected; and the works, in which he has made them public, are so many master-pieces of sagacity and of methodical reasoning; each perfect as a whole and in its parts, and leaving nothing for any other hand to correct, but rising in splendour with each successive year that passes over them, and promising to carry down his name to a posterity far more remote than his rank and connections could ever have enabled him to attain without them."

In his manners Mr. Cavendish had the appearance of a quickness and sensibility almost morbid, united to a slight hesitation in his speech, which seems to have depended more on the constitution of his mind than on any deficiency of his organic powers, and to an air of timidity and reserve, which sometimes

afforded a contrast, almost ludicrous, to the sentiments of profound respect which were professed by those with whom he conversed. It is not impossible that he may have been indebted to his love of severe study, not only for the decided superiority of his faculties to those of the generality of mankind, but even for his exemption from absolute eccentricity of character. His person was tall, and rather thin: his dress was singularly uniform, although sometimes a little neglected. His pursuits were seldom interrupted by indisposition; but he suffered occasionally from calculous complaints. His retired habits of life, and his disregard of popular opinion, appear to have lessened the notoriety which might otherwise have attached to his multiplied successes in science; but his merits were more generally understood on the Continent than in this country; although it was not till he had passed the age of seventy, that he was made one of the eight Foreign Associates of the Institute of France.

Mr. Cavendish was no less remarkable in the latter part of his life, for the immense accumulation of his pecuniary property, than for his intellectual and scientific treasures. His father died in 1783; being at that time eighty years old, and the senior member of the Royal Society: but he is said to have succeeded at an earlier period to a considerable inheritance left him by one of his uncles. He principally resided at Clapham Common; but his library was latterly at his house in Bedford Square; and his books were at the command of all men of letters, either personally known to him, or recommended by his friends: indeed the whole arrangement was so impartially methodical, that he never took down a book for his own use, without entering it in the loan book; and after the death of a German gentleman, who had been his librarian, he appointed a day on which he attended in person every week for the accommodation of the few, who thought themselves justified in applying to him for such books as they wished to consult. He was constantly present at the meetings of the Royal Society, as well as at the conversations held at the house of the President; and he dined every Thursday with the club composed of its members. He had little intercourse with general society, or

even with his own family, and saw only once a year the person whom he had made his principal heir. He is said to have assisted several young men, whose talents recommended them to his notice, in obtaining establishments in life; but in his later years, such instances were certainly very rare. His tastes and his pleasures do not seem to have been in unison with those, which are best adapted to the generality of mankind; and amidst the abundance of all the means of acquiring every earthly enjoyment, he must have wanted that sympathy, which alone is capable of redoubling our delights, by the consciousness that we share them in common with a multitude of our friends, and of enhancing the beauties of all the bright prospects that surround us, when they are still more highly embellished by reflection "from looks that we love." He could have had no limitation either of comfort or of luxury to stimulate him to exertion; even his riches must have deprived him of the gratification of believing, that each new triumph in science might promote the attainment of some great object in life that he earnestly desired; a gratification generally indeed illusory, but which does not cease to beguile us till we become callous as well to the pleasures as to the sorrows of existence. But in the midst of this "painful pre-eminence," he must still have been capable of extending his sensibility over a still wider field of time and space, and of looking forwards to the approbation of the wise and the good of all countries and of all ages: and he must have enjoyed the highest and purest of all intellectual pleasures, arising from the consciousness of his own excellence, and from the certainty that, sooner or later, all mankind must acknowledge his claim to their profoundest respect and highest veneration.

"It was probably either the reserve of his manners," says Cuvier, "or the modest tone of his writings, that procured him the uncommon distinction of never having his repose disturbed either by jealousy or by criticism. Like his great countryman Newton, whom he resembled in so many other respects, he died full of years and honours, beloved even by his rivals, respected by the age which he had enlightened, celebrated throughout the scientific world, and exhibiting to mankind a perfect model of

what a man of science ought to be, and a splendid example of that success, which is so eagerly sought, but so seldom obtained." The last words that he uttered were characteristic of his unalterable love of method and subordination: he had ordered his servant to leave him, and not to return till a certain hour, intending to pass his latest moments in the tranquillity of perfect solitude: but the servant's impatience to watch his master diligently having induced him to infringe the order, he was severely reproved for his indiscretion, and took care not to repeat the offence until the scene was finally closed. Mr. Cavendish died on the 24th of February, 1810; and was buried in the family vault at Derby. He left a property in the funds of about 700,000*l.*, which he divided into six equal parts, giving two to Lord George Cavendish, the son of his first cousin, one to each of his sons, and one to the Earl of Besborough, whose mother was also his first cousin. Some other personal property devolved to Lord George as residuary legatee; and a landed estate of 6,000*l.* a year descended to his only brother, Mr. Frederic Cavendish, of Market Street, Herts, a single man, and of habits of life so peculiarly retired, that any further increase of income would have been still more useless to him than it had been to the testator.

Much as Mr. Cavendish effected for the promotion of physical science throughout his life, it has not been unusual, even for his warmest admirers, to express some regret that he did not attempt to do still more after his death, by the appropriation of a small share of his immense and neglected wealth, to the perpetual encouragement of those objects, which he had himself pursued with so much ardour. But however we might be disposed to lament such an omission, we have surely no reason to complain of his determination to follow more nearly the ordinary course of distribution of his property, among those whose relationship would have given them a legal claim to the succession, if he had not concerned himself in directing it. We may observe on many other occasions, that the most successful cultivators of science are not always the most strenuous promoters of it in others; as we often see the most ignorant persons, having been rendered sensible by experience of their own

deficiencies, somewhat disposed to overrate the value of education, and to bestow more on the improvement of their children than men of profounder learning, who may possibly have felt the insufficiency of their own accomplishments for insuring success in the world. But even if Mr. Cavendish had been inclined to devote a large share of his property to the establishment of fellowships or professorships, for the incitement of men of talents to a more complete devotion of their lives to the pursuit of science, it is very doubtful whether he could have entertained a reasonable hope of benefiting his country by such an institution: for the highest motives that stimulate men to exertion are not those which are immediately connected with their pecuniary interests: the senators and the statesmen of Great Britain are only paid in glory; and where we seek to obtain the co-operation of the best educated and the most enlightened individuals in any pursuit or profession, we must hold out as incentives the possession of high celebrity and public respect; assured that they will be incomparably more effectual than any mercenary considerations, which are generally found to determine a crowd of commercial speculators to enter into competition for the proposed rewards, and to abandon all further concern with the objects intended to be pursued, as soon as their avarice is gratified. To raise the rank of science in civil life is therefore most essentially to promote its progress: and when we compare the state, not only of the scientific associations, but also of the learned professions in this country and among our neighbours, we shall feel little reason to regret the total want of pecuniary patronage that is remarkable in Great Britain, with respect to every independent department of letters, while it is so amply compensated by the greater degree of credit and respectability attached to the possession of successful talent. It must not however be denied, that even in this point of view there might be some improvement in the public spirit of the country: Mr. Cavendish was indeed neither fond of giving nor of receiving praise; and he was little disposed to enliven the intervals of his serious studies by the promotion of social or convivial cheerfulness: but it would at all times be very easy for an individual, possessed of high rank and

ample fortune, of correct taste and elegant manners, to confer so much dignity on science and literature by showing personal testimonies of respect to acknowledged merit, as greatly to excite the laborious student to the unremitting exertions of patient application, and to rouse the man of brilliant talent to the noblest flights of genius.

No. LXIX.

LIFE OF SMITHSON TENNANT.

SMITHSON TENNANT, a distinguished chemist, born at Selby, in Yorkshire, 30th November, 1761, was the only child of the Rev. Calvert Tennant, younger son of a respectable family in Wensley Dale, near Richmond, and vicar of Selby; his mother was Mary Daunt, daughter of a surgeon of that town.

His father had been a fellow of St. John's College, Cambridge, and began to teach his son Greek when he was only five years old. He had the misfortune to lose him four years after; and before he grew up, his mother also, while he was riding with her, was thrown from her horse and killed on the spot. He was sent, after his father's death, to different schools, at Scorton, near Richmond; Tadcaster, and Beverley; in these he was remembered as a boy retired in his manners, and somewhat melancholy, and indolent with respect to puerile amusements. He learned but little at school, and may be considered as in great measure self-educated; having been fond, almost as a child, of reading books of science, and of amusing himself with little experiments which he found described in them; and while he was at school at Tadcaster, he took great delight in attending a course of Walker's lectures on experimental philosophy which were given there. At Beverley he was under the care of Dr. G. Croft, who had made himself known to the public by some controversial writings; here he never entered much into the pursuits of his contemporaries, but profited by a good library belonging to the school; and among other books which he read with avidity was Sir Isaac Newton's *Treatise on Optics.*

He had entertained a great desire to complete his chemical studies under the immediate instruction of Dr. Priestley, who was then enjoying deserved reputation for his recent experi-

mental discoveries; but Dr. Priestley's occupations did not permit him to undertake the task of directing his education, however agreeable it might have been to him to have assisted such a pupil. In the mean time he had not neglected his classics, but had acquired a sufficient knowledge of the learned languages to appreciate with correct taste the beauties of the great writers of antiquity. Notwithstanding his admiration for Dr. Priestley, he was an early convert to the antiphlogistic theory of chemistry; which, with all its errors, was still a material step in the advancement of science.

In 1781 he went to Edinburgh, with the view of qualifying himself for the profession of physic, and he had the advantage of attending Dr. Black's lectures, which were then in great reputation. In October, 1782, he entered as a pensioner of Christ's College, Cambridge, where he became intimately acquainted with the late Professor Harwood, who had been first a surgeon in India, but having lost, by the misconduct of an agent, the fortune that he had there acquired, submitted cheerfully to the toil of recommencing his career as a physician, though already past the middle age; his talents for conversation were such as were extremely likely to captivate a young man of superior discernment; and he formed a friendship with Tennant which continued uninterrupted throughout their lives.

At Cambridge he studied a little of the mathematics, in the works of Newton, but much more of chemistry and botany: he already began to exercise his inventive powers in an attempt to economise the consumption of fuel in distillation, which he did not make public until twenty years after, though he mentioned it at the time to some of his friends. He also occupied himself incessantly in general and especially in political, reading, though he was far from having the air of a student; but his rooms were always in confusion from the mixture of heterogeneous materials that were accumulated in them. His residence at Cambridge was perhaps the happiest time of his life; his spirits unwearied, his health unbroken, his feelings acute, and his conversation brilliant, though simple and unaffected.

In the summer of 1784 he paid a visit to Denmark, to Sweden,

and to Scheele, whose acquaintance extremely delighted him, and most of all from the simplicity of the apparatus that he employed in his researches. A year or two afterwards he went to France, and being taken ill at Paris, he was joined there by his friend Sir Basil Harwood, with whom he returned through Holland and the Netherlands, at the time that the bigoted people of the Low Countries were in insurrection against a philosophical despot, while Holland remained free and prosperous.

He was particularly intimate with Dr. Milner, the Master of Queen's College, and was recommended by his signature, together with those of Waring, Maskelyne, Jebb, and Watson, as a Fellow of the Royal Society, into which he was admitted in January, 1785. He removed, together with his friend Harwood, in December, 1786, from Christ's College to Emmanuel, and in 1788 he took a degree of Bachelor of Physic.

In 1791 he communicated to the Royal Society his very interesting discovery of a mode of obtaining carbon from the carbonic acid. Having observed that charcoal did not decompose the phosphate of lime, he concluded that phosphorus ought to decompose the carbonate of lime, and the result fully justified his manner of reasoning.

He paid a third visit to the continent in 1792, intending to go on through France into Italy, and arrived at Paris not long before the 10th of August; but he saw some indications of an impending convulsion, and was fortunate enough to quit Paris on the 9th. He visited Gibbon at Lausanne, and was much interested in the sagacity that this eminent writer displayed in his conversation. He went on to Rome and Florence, where he was fully impressed with all the admiration that he had been taught to anticipate for the treasures of ancient and modern art possessed by those cities; and, in his return through Germany, he was greatly amused by the mixture of knowledge and credulity that he observed among the studious of that country. At Paris he found everything enveloped in gloom and overwhelmed with terror, in 1793; his friend Lametherie was alarmed by the visit which he paid him, but he had the integrity to preserve for him entire some property of considerable value, with which he had entrusted him.

Upon his arrival in London, Mr. Tennant took chambers in the Temple, and was in the habit of living much with some of his early acquaintance, who had adopted the law as their profession; to his own he was in great measure indifferent, neither seeking to practise it, nor being well calculated to succeed greatly in it with the public, though he studied it with attention, and took pains to make himself master of its history and its philosophy, being a particular admirer of Sydenham, when considered in relation to the age in which he lived. He took his degree of Doctor of Physic in 1796, and in the same year he gave the Royal Society a paper on the quantity of carbonic acid afforded by the diamond, which he measured by heating it with nitre, and obtaining a precipitate by the addition of muriate of lime; and he found that the diamond afforded no more carbonic acid than an equal weight of charcoal. A subsequent communication contained the result of his observations on the action of heated nitre on gold and platina.

The love of travelling appeared to be his predominant passion; he studied in his travels, not only the natural and political history of the countries that he saw, but also their languages, and the philosophy of their etymologies. He observed, too, the peculiarities of their agriculture, and, in 1797, he determined, after visiting an agricultural friend in Lincolnshire, to devote his attention to practical farming as a serious pursuit. He purchased some allotments of unenclosed land in that neighbourhood, but he left the management of them chiefly to his friend, and made afterwards considerable additions to the property by further purchases. In 1798 or 1799, he bought a tract of newly enclosed land on the Mendip hills, near Chedder, where he built a house, and resided for some months every summer through the remainder of his life. These speculations, though their results were at first doubtful, yet succeeded remarkably well on the whole; more especially considering the benefit that his health derived from the travelling and the exercise that they rendered necessary: but they occupied too much of his attention, and of that time which might have been employed so much more to the advantage of the public, and to his own ultimate satisfaction.

In 1799 he gave the Royal Society a paper on the magnesian limestone, or dolomite, which he considers as rather a combination than an accidental mixture; and the forms of the crystals, as they have been determined by later observers, together with the laws of definite proportions, have tended to confirm this conjecture; he found that grain will scarcely germinate, and soon perishes, when sown in the neutral carbonate of magnesia. In 1802 he published his paper on emery, which he showed to be a substance similar to the corundum or adamantine spar of China, and not an ore of iron, as had been commonly supposed. In the month of July he was making some experiments on crude platina, when he discovered in it a singular dark powder, which was left undissolved by the nitromuriatic acid, and which was also observed the next year by Messrs. Descotils and Vauquelin. In 1804 Mr. Tennant showed that the powder contained two new metals, which he named *Iridium* and *Osmium;* and he received the Copleian medal from the Royal Society in November, as an acknowledgment of the merit of his various chemical discoveries. In 1805 and 1806 he paid two successive visits to Ireland, by way of Scotland, one of them in company with Browne the traveller, for whom he had a high esteem, and to whom he suggested the observation of the temperature of boiling water as a mode of determining the heights of mountains; a method, however, which had been long before recommended by Achard and others.

He became latterly more fond of general society than he had been in his earlier years, and he used to receive miscellaneous parties at his chambers, and to show them prints, and minerals, and novelties of various kinds. In 1812 he was persuaded to convert these mixed exhibitions into a more regular course of lectures, principally upon mineralogy, calculated especially for the ladies of his acquaintance, and which highly delighted all his audience: "Their attention was perpetually kept alive by the spirit and variety with which every topic was discussed, by anecdotes and quotations happily introduced, by the ornaments of a powerful but chastised imagination, and, above all, by a peculiar vein of pleasantry, at once original and delicate, with which he could animate and embellish the most unpromising

subjects;"—a circumstance which, though not of much immediate importance to the public, yet probably led him the more readily to accept a Professorship at Cambridge, and would thus, if he had survived longer, have greatly extended the sphere of his utility.

He delivered, in 1813, a lecture on mineralogy to the Geological Society, and gave them also an account of his analysis of a volcanic substance from the Lipari Islands containing the boracic acid, which has since been examined on the spot by Dr. Holland. In the month of May he was elected Professor of Chemistry in the University of Cambridge, all opposition having been withdrawn before the election. The following spring he gave his first and last course of lectures there. His introductory lecture still exists in manuscript, and is said to contain a masterly sketch of the history of the science. He communicated to the Royal Society, in 1814, a paper on the easiest mode of procuring potassium, and another on the economy of heat in distillation, proposing to heat a second boiler by the condensation of the steam of the first. In the spring and summer of this year he was occupied in searching for the origin of iodine, and he succeeded in detecting this substance in sea-water, by the test of its tarnishing the surface of leaf-silver. One of the last services that he rendered the Royal Society, was in the capacity of a member of a committee which was formed in order to investigate, at the request of the Government, the degree of danger that might attend the general introduction of gas lights into the metropolis. He undertook, together with his friend Dr. Wollaston, to make some experiments upon the inflammation of the gas, and they discovered conjointly the very important fact, that the gas contained in a small tube will not communicate the flame; a fact which, in the hands of Sir Humphry Davy, has been rendered productive of consequences so important to the public safety; although Sir Humphry having been abroad at the time of this investigation, and the report of the committee not having been then published, he had to rediscover this truth, and many more, in his most ingenious and successful researches.

It was early in the month of September that Mr. Tennant

went for the last time to France, being impatient to observe the changes that an eventful interval of twenty years had produced in that highly interesting country He was greatly delighted with Lyons and Marseilles, and, returning to Paris in November, he lingered there till February, 1815; on the 15th of that month he arrived at Calais; the 20th he went to Boulogne with Baron Bulow, in order to embark there; they did embark on the 22nd, but were forced back by the wind, and meant to try again in the evening; in the mean time they took horses and went to see Bonaparte's Pillar, about a league off, and going off the road on their return, to look at a small fort, of which the drawbridge wanted a bolt, they were both thrown, with their horses, into the ditch. Bulow was only stunned, but Tennant's skull was so severely fractured, that he died an hour after.

His papers published in the *Philosophical Transactions* were eight in number. 1. *On the Decomposition of Fixed Air*, 1791, p. 182. 2. *On the Nature of the Diamond*, 1797, p. 123. 3. *On the Action of Nitre upon Gold and Platina*, p. 219. 4. *On the different sorts of Lime used in Agriculture*, 1799, p. 305. 5. *On the Composition of Emery*, 1802, p. 398. 6. *On two Metals found in the Black Powder remaining after the Solution of Platina*, 1804, p. 411. 7. *On an easier Mode of procuring Potassium than that which is now adopted*, 1814, p. 578. 8. *On the means of producing a double Distillation by the same Heat*, p. 587.

9. The analysis of *A Volcanic Substance containing the Boracic Acid*, appeared in the *Transactions of the Geological Society*, Vol. I., 1811.

Mr. Tennant was tall and slight in his person; his face was thin, and his complexion light; he resembled a little the portraits of Locke; he was generally negligent in his dress; but on the whole, agreeable in his appearance. He was distinguished for good sense, for quickness of perception, and for penetration; but, as his friend and biographer Mr. Whishaw observes, in the admirably energetic sketch that he has given of his character, he was one of those who, to use the words of Dr. Johnson, "without much labour, have obtained a high

reputation, and are mentioned with reverence rather for the possession than the exertion of uncommon abilities." "His curiosity and activity were incessant; he had a vigilance of observation which suffered nothing to escape him, and was continually gaining new information from a variety of interesting sources. But although the knowledge thus acquired was remarkable for its correctness, and complete for the purposes of its possessor, yet the industry and perseverance, by which it ought to have been embodied and made permanent for the benefit of others, were too often altogether wanting. The ardour and energy of Mr. Tennant's mind co-operated, unfortunately, in this respect, with his want of method and of systematic habits of application; since he was constantly pressing on to new discoveries, instead of arranging and bringing to perfection those which he had already made. His memory was a great storehouse of discoveries, and hints for discovery, of ascertained facts, probable conjectures, and ingenious trains of reasoning relative to the various important subjects upon which he had at any time been engaged. These he was continually treasuring up, with the intention of reducing them to order, and preparing them for use at a more convenient season. But that period rarely arrived. In the carelessness of intellectual wealth, he neglected those stores of knowledge which he had accumulated, and suffered them to remain useless and unproductive, till his attention was recalled to them, perhaps after a long course of years, by some new fact or discovery, some remark in conversation, or other accidental occurrence."

The effect of his peculiar cast of humour was heightened by a perfect gravity of countenance, a quiet familiar manner, and a characteristic simplicity of language. He was firmly attached to the general principles of freedom, being fully convinced "of their influence in promoting the wealth and happiness of nations; a due regard to these principles he considered as the only solid foundation of the most important blessings of social life, and as the peculiar cause of that distinguished superiority, which our own country so happily enjoys among the nations of Europe." "The cheerful activity of a populous town, the improvements in the steam-engine, the great galvanic experiments, and above

all, the novelty and extent of the prospects afforded by that revolution in chemical science which has illustrated our own age and country; these magnificent objects, when presented to Mr. Tennant's mind, excited in him the liveliest emotions, and called for the most animated expressions of admiration and delight." "He thought himself passionate and irascible; and certainly his feelings were quick, but they were transitory." He possessed a strong sense of high honour, as well as of duty; and his liberality and humanity were evinced by some practical occurrences in which he had occasion to exercise them; his steward had defrauded him, and when the day of reckoning came, had destroyed himself: he not only forgave the debt, but provided also for the widow and her family."

"His amiable temper and unaffected desire of giving pleasure, no less than his superior knowledge and talents, had rendered him highly acceptable to a numerous and distinguished circle of society, by whom he was justly valued, and by whom his premature death was sincerely lamented. But the real extent of his private worth, the genuine simplicity and virtuous independence of his character, and the sincerity, warmth, and constancy of his friendship, can only be felt and estimated by those to whom he was long and intimately known, and to whom the recollection of his talents and virtues must always remain a pleasing though melancholy bond of union."

No. LXX.

LIFE OF SIR BENJAMIN THOMPSON,

COUNT RUMFORD.

SIR BENJAMIN THOMPSON, COUNT RUMFORD, a well-known Natural Philosopher and Political Economist, was born in 1753, at a village in New Hampshire, then called Rumford, and now Concord.

His father died while he was very young, and his mother married another man, who banished him from her house almost in his infancy; he inherited only a small pittance from an uncle, who died soon after his father. A clergyman named Bernard showed him great kindness, and taught him some of the higher mathematics at an early age, so that at fourteen he was able to calculate and delineate an eclipse of the sun. He had been intended for some commercial employment, but he preferred the pursuit of literature in any form; he attended the lectures of Dr. Williams, and afterwards those of Dr. Winthrop, the astronomer, at the College of "Harvard;" and while he was still a stripling, he was established in the temporary occupation of a village schoolmaster; hoping, however, for an early opportunity to engage in some more agreeable employment; and at nineteen he was fortunate enough to obtain the hand of Mrs. Rolfe, daughter of Mr. Walker, a clergyman who had been employed with considerable credit in conducting some public business. For a year or two he lived retired and happy, but having obtained a commission of Major in the Militia from the Governor of the province, together with some other distinctions, of a civil nature, he was consequently led to adhere to the party of the Royalists, and he was soon obliged, by the success of the Independent forces, to take refuge at Boston, then occupied by the English troops. It was in November 1773, that he secretly

quitted his residence, leaving his wife, whom he never saw again, and his infant daughter, who joined him twenty years after in Europe.

He was employed to raise a regiment for the King's service; but when Boston was evacuated, in 1776, he was sent with some important dispatches to England. Here he soon acquired the confidence of Lord George Germaine, then Colonial Secretary of State, and was appointed Secretary of the province of Georgia, though he never exercised the office; but he remained attached to that department of the public service.

In 1777 he commenced his career as an experimental philosopher, by employing his leisure hours, during a visit to Bath, in making some experiments on the cohesive strength of different substances; and upon his return to London, he communicated them to Sir Joseph Banks, with whom he formed an intimate acquaintance, which he kept up throughout the remainder of his life. In 1778 he was admitted a Fellow of the Royal Society, and he made in that year his first experiments on gunpowder. In order to pursue these experiments, he went, in 1779, on board of the Victory of 110 guns, commanded by his friend Sir Charles Hardy. He passed the whole of the campaign on board of the fleet; and the results of the observations, that he then made, furnished the materials of a chapter which he contributed to Stalkart's Treatise on Naval Architecture. He added to it a code of signals for the Navy, which was not published. In 1780 he was appointed Under Secretary of State, and he was constantly employed, for some little time, in the office on the business of the war. He succeeded, by means of his American friends and agents, in raising a regiment of cavalry, called the King's American Dragoons, of which he was appointed Lieutenant-Colonel Commandant, and this success induced him to go to America to serve with it. At Charlestown he was entrusted with the command of the remains of the cavalry of the British army;—he speedily restored the discipline of the corps, and gained its confidence and attachment: he often led it on against the enemy, and frequently with considerable success. He proceeded, in 1782, to New York, where he assumed the command of his own regiment, having received

the colours from the hand of Prince William Henry, now Duke of Clarence. In the autumn, General Clinton was succeeded by Sir Guy Carlton, whose friendship and confidence he speedily obtained: his regiment was recruited from the fragments of several others, and he was sent for the winter to Huntingdon in Long Island. He was chosen, in 1783, to conduct the defence of Jamaica, which was then threatened by the enemy; but the general peace superseded the necessity of the intended expedition.

After his return to England, he made great efforts in the cause of the loyalist officers, and he was successful in persuading the Ministry to make a proper provision for them: he was himself made a full Colonel, upon the recommendation of General Carlton, only two years after his appointment as Lieutenant-Colonel. He had acquired a strong predilection for a military life, and was desirous of being sent with his regiment to the East Indies; and when the regiment was reduced, he wished to serve with the Austrians in a war which was then meditated against the Turks.

With this view he left England in September, 1783, and on his passage to Boulogne, he had an agreeable ship-mate in the person of Gibbon, the historian, who did justice to his merits as a "soldier, philosopher, and statesman." At Strasburg, his appearance on the parade in his uniform excited the attention of the present King of Bavaria, then Prince Maximilian of Deux Ponts, who invited him to his table, and being delighted with the accuracy and extent of his military knowledge, gave him a strong recommendation to his uncle, then Elector; and instead of a day or two, as he had intended, he staid a fortnight at Munich. He was also very cordially received at Vienna, and passed a part of the winter there; but the war against the Turks not taking place, he went by Venice and the Tyrol again to Munich, where he arrived in the winter of 1784; and being formally invited by the Elector to enter his service, he went to London to ask leave to accept the proposal, and it was granted him, together with the honour of knighthood. On his return to Bavaria, he was made a Colonel of cavalry, and Aide-de-camp-General to the Elector. The first four years of his residence

at Munich were principally employed in acquiring information, and in preparing his plans of reform; and in the mean time he continued his physical researches. He made his first experiments on heat in 1786, during a journey to Mannheim. In 1785 he was made Chamberlain to the Elector, and member of the Academies of Munich and of Mannheim; in 1786 he received from the King of Poland the Order of St. Stanislaus; in 1787 he took a journey to Berlin, and was made a member of the Academy of Sciences of that city; in 1788 he was appointed Major-General of the Bavarian cavalry, and Privy Counsellor of State; and he was placed at the head of the War department, in order to pursue his plans for the improvement of the army.

It was in 1789 that he established the House of Industry at Mannheim; he founded also the Military Academy of Munich; he improved the military Police of the country; he formed Schools of Industry for the wives and children of the soldiers; and he embellished the city by a new arrangement of the public gardens. The House of Industry at Munich, which he has described at large in his *Essays*, was founded in 1790; and from this period may be dated the total abolition of mendicity in Bavaria. His exertions were rewarded by the rank of Lieutenant-General of the Bavarian armies, and by a regiment of artillery. In 1791 he was created a Count of the Holy Roman Empire, and obtained the Order of the White Eagle.

His health having suffered from constant application, he obtained permission to take a journey into Switzerland and Italy, and he returned to Bavaria in 1794. He had a severe illness at Naples, and he was not sufficiently recovered upon his return to resume his active duties; but he employed himself in writing the first five of his *Essays*. In 1795 he came to England in order to publish the *Essays*, and in hopes of exciting the public atttention to the importance of attempting a similar reform among the lowest orders in Great Britain. He went to Dublin in 1796, to pay a visit to Lord Pelham, now Earl of Chichester, then Secretary of State for Ireland; and he was of essential service in the arrangement of several of the public institutions of that country. He was made a member of the

Royal Irish Academy, and of the Society for the Encouragement of Arts; and he received, after having left the country, the public thanks of the Grand Jury of the county of Dublin, and of the Lord Mayor of that city, as well as of the Lord Lieutenant at the head of the Government. Upon his return to London, he superintended some improvements at the Foundling Hospital, and presented several models of machines and implements to the Board of Agriculture; and he established two prizes for discoveries relating to Heat and Light, by placing two sums of 1000*l.* in the British and in the American funds, to be adjudged biennially, for Europe by the Royal Society of London, and for America by the American Academy of Sciences.

He was recalled to Bavaria by the exigencies of the moment, which were such as to cause the Elector to take refuge in Saxony; General Moreau having advanced with his army to the confines of Bavaria. After the battle of Friedberg, Count Rumford was left in command of the Bavarian army, with instructions to act according to his discretion under the circumstances that might occur; and his firmness was such, as to prevent either the Austrians or the French from entering Munich. On the Elector's return, he was placed at the head of the department of the general police of Bavaria. His exertions in this office were such, as to impair the state of his health, and by way of an honourable retirement, he was sent to England in the capacity of Envoy Extraordinary and Minister Plenipotentiary; but being a subject of the King of England, he was judged incapable of being received as the diplomatic agent of a foreign court, and he, therefore, continued to live in England as a private individual.

He was very active about this time in projecting and superintending the establishment of the Royal Institution of Great Britain, which was more particularly intended for the application of science to the conveniences and comforts of civil and domestic life, but which has been no less successful in giving opportunity and facility to some of the most refined researches in chemistry and natural philosophy that have distinguished the age, than in serving as a medium for making the treasures of science accessible to the less studious part of the public, and as a model for

a variety of other similar undertakings in different parts of the world.

Count Rumford was soon afterwards officially invited to America by the Government of the United States, with an offer of an honourable establishment in a public situation; but he considered it as inconsistent with his engagements in Europe to accept the proposal. In the autumn of 1800, when he went to Scotland, a visit of ceremony was paid him by the magistrates of Edinburgh; he was consulted respecting the abolition of mendicity, and the measures that he recommended were speedily executed with complete success. He was made an honorary member of the Royal Society, and of the Royal College of Physicians of Edinburgh; and he received a gold snuff box as a compliment for his assistance in reforming the culinary establishment of Heriot's Hospital.

After so active and diversified a career, it was not to be expected that he would be satisfied with the monotony of a permanent residence in London; he was so accustomed to labour for the attainment of some object, that when the object itself was completely within his reach, and the labour was ended, the prospect, which ought to have been uniformly bright, became spontaneously clouded, or even the serenity became unenjoyable for want of some clouds to afford a contrast. He had fitted up a small house at Brompton, with every contrivance for comfort and convenience that could render it fit for the abode of hospitality and of luxury, and the arrangements are fully described in the *Bibliothèque Britannique* of his friend Professor Pictet; but after all he never was known to give a single entertainment in it. The enthusiasm excited by the novelty of some of his inventions had subsided, and he was even mortified by becoming, in common with the most elevated personages of the country, the object of the impertinent attacks of a popular satirist. It was partly, however, if not entirely, the superiority of the climate of France, that determined him to remove to Paris in the spring of 1802; he went in the summer to Munich, and the following year he made a tour in Switzerland and in Bavaria, accompanied by Madame Lavoisier, whom he married soon after their return; but their habits were incompatible with matri-

monial comfort, and they separated soon after; Count Rumford retiring to Auteuil, about four miles from Paris, where he occupied a house which had formerly belonged to Helvetius and to Cabanis, while his lady continued to live in the metropolis. His latter years were passed almost wholly in solitude; he saw only his neighbour Mr. Caneleux, Mr. Underwood, and a Mr. Parker, an American; he did not even attend the sittings of the Institute, though he had been made one of its eight foreign associates some time before, and always retained a high esteem for its secretary, Cuvier, and for some others of its members. His income was abundantly sufficient for his own expenses, having obtained from the gratitude of the king of Bavaria a pension of 1200*l.* a year; and he was allowed by Bonaparte to remain unmolested, though a British subject, when it was found that he had no intercourse with society, amusing himself principally in walking about his garden, and in a solitary game of billiards. He had so completely persuaded himself, in the latter part of his life, of the great superiority of broad wheels above narrow ones, that he drove about the streets of Paris in a broad wheeled chariot; and having rediscovered, after Professor Leslie, that black bodies radiate more heat than others, he wore, in the winter, a white hat and a white coat, in order to economize the heat of his person. "These peculiarities, and a peremptory, unyielding disposition," says one of his biographers, "were the causes that set him apart from social intercourse, and in all his connections in life, seem to have rendered him less the object of personal attachment than of esteem for his talents and activity." He was about to return to England when he died, the 21st August, 1814, leaving only one daughter, who still resides in his house at Brompton.

1. Count Rumford's first publication appears to have been the chapter on *Marine Artillery* that he furnished to Stalkart's *Treatise on Naval Architecture*, 1780.

2. *New Experiments on Gunpowder, with the Description of an Eprouvette. Phil. Trans.* 1781. P. 230. The effect of the powder on the ball was measured by the recoil of the piece, with a correction deduced from the recoil when the piece was

empty. It was observed to be sooner heated when fired without ball than with it. The force of the powder is made at least 1300 atmospheres, upon Robins's principles.

3. *New Experiments upon Heat. Phil. Trans.* 1786. P. 273. These experiments relate principally to the conducting powers of various mediums for heat; but the results are unavoidably complicated with the effects of radiation, in consequence of which a vacuum is supposed to possess a conducting power more than half as great as that of common air.

4. *Experiments on the Production of Dephlogisticated Air from Water with various Substances. Phil. Trans.* 1787. P. 84. These experiments tend to show that the air obtained, by Priestley and Ingenhousz, from plants under water, was derived rather from the water itself, than from the substances immersed in it.

5. *Experiments made to determine the Positive and Relative Quantities of Moisture absorbed from the Atmosphere by various Substances.* P. 240. He finds that wool is more absorbent of moisture than any other substance compared with it; and hence explains the supposed advantage of woollen worn next the skin.

6. *Experiments on Heat. Phil. Trans.* 1792. P. 48. The author attributes the effect of loose substances in obstructing the passage of heat to their attraction for air, and to their impeding its circulation; and he supposes this to be the only manner in which elastic fluids communicate heat.

7. *Account of a Method of Measuring the Comparative Intensity of Light emitted by Luminous Bodies. Phil. Trans.* 1794. P. 67. Placing them at such distances, that the shadows cast by each may appear equally dark.

8. *Letter announcing a Donation for a Prize Medal. Phil. Trans.* 1797. P. 215.

9. *Experiments to determine the Force of Fired Gunpowder.* P. 272. This force he supposes to amount to between 20,000 and 50,000 atmospheres, instead of 10,000, as Bernoulli computed it; but he makes a great mistake in supposing that the whole of the water which can possibly be contained in the gunpowder would be sufficient to furnish as much steam as

would be required, since steam, under a pressure of 20,000 atmospheres, must be considerably more dense than water itself.

10. *Inquiry concerning the Source of the Heat excited by Friction. Phil. Trans.* 1798. P. 80. The capacity of the chips of iron afforded by friction in boring a cannon, was found not to differ from that of the iron in its original state: hence it is inferred that the heat could not have been furnished by them, and that it must probably have been generated. Mr. Haldalt afterwards repeated the experiment under circumstances still more decisive; and Sir Humphry Davy showed that two pieces of ice rubbed together, in a room below the freezing temperature, would melt each other.

11. *Inquiry concerning the Chemical Properties that have been attributed to Light.* P. 449. He attributes these properties to the effect of an intense heat confined to a small space; but the later experiments on the chemical effects of the spectrum are sufficient to supersede this opinion.

12. *An Account of a Curious Phenomenon observed on the Glaciers of Chamouny, with some observations on the Propagation of Heat in Fluids. Phil. Trans.* 1804. P. 23. An effect depending on the expansion of water in cooling near the freezing point.

13. *Concerning the Nature of Heat, and the Mode of its Communication.* P. 77. He conjectures that cold is a positive quality, capable of being propagated by radiation.

Several of these memoirs were reprinted under the title of *Philosophical Papers.* Vol. I. 8. Lond. 1802.

14. The *Essays* constitute 4 vols. 8vo. Lond. 1795 . . 1800. Reprinted 1800. In French, 2 v. 8. Genev. 1799. *Recueil de Rapports* . . sur les Soupes. Par. 1801. They are 18 in number.

i. Account of an Establishment for the Poor at Munich. In Ital. 8. Venice, 1798.
ii. On Establishments for the Poor in general.
iii. Of Food, and of Feeding the Poor.
iv. Of Chimney Fireplaces.
v. Account of several Public Institutions formed in Bavaria.
vi. On the Management of Fire, and the Economy of Fuel.

vii. On the Propagation of Heat in Fluids, extending to Liquids the doctrine which he had before advanced respecting Elastic Fluids.

viii. On the Propagation of Heat in various Substances. *Phil. Trans.*

ix. Experimental Inquiry concerning the Source of Heat excited by Friction.

x. On Kitchen Fireplaces and Kitchen Utensils.

xi. On Chimney Fireplaces.

xii. On the Salubrity of Warm Rooms in Cold Weather.

xiii. On the Salubrity of Bathing, and the Construction of Warm Baths.

xiv. Supplementary Observations on the Management of Fires.

xv. On the Use of Steam for Transporting Heat.

xvi. On the Management of Light in Illumination, with an Account of a New Portable Lamp.

xvii. On the Source of the Light manifested in the Combustion of Inflammable Bodies.

xviii. On the Excellent Qualities of Coffee, and the Art of making it in Perfection.

15. There are several little papers on *Steam Kitchens*, on the *Strength of Soft Materials*, and on some other similar subjects, in the first numbers of the *Journals* of the Royal Institution. 8. Lond. 1800.

16. The series of investigations relating to Heat and Light, which Count Rumford began to communicate to the Royal Society, were continued, and rather more fully detailed, in several of the volumes of the *Memoirs of the Institute,* Mathematical class; into which thev were, of course, admitted as the productions of a Foreign Associate. The first of these is in the sixth volume. 4. Par. 1806. P. 71, containing a *Description of a New Instrument;* a thermoscope, or a differential thermometer, resembling that of Leslie. (17.) The second, P. 74. *Researches on Heat;* showing the effect of the difference of surface on radiation. (18.) iii. P. 88. *Further Experiments,* in the effect of blackening the surface. (19.) iv. P. 97. *Researches Continued;* on the different properties of bodies with respect to radiation, and to conducting power. (20.) v. P. 106. *Further Researches;* some good experiments on the passage of heat through solids. (21.) vi. P. 123. *Experiments*

on the Heat of the Solar Rays; which was found not to be affected by their convergence or divergence, or by their having met in a focus and crossed each other. (22.) vii. Vol. VII. i. 1806. P. 78. *Remarks on the Temperature of Water at the Maximum of Density;* making it 41° of Fahrenheit, or 5° centigrade. *Phil. Trans.* 1804. (23.) viii. Vol. VIII. i. 1807. P. 223. *On the Dispersion of the Light of Lamps by Screens of Ground Glass, Silk, and so forth, with a Description of a New Lamp.* (24.) ix. P. 249. *On the Cooling of Liquids in Vases of Porcelain, gilt and not gilt;* showing the utility of gilding them externally, with some good reasoning on the nature of heat.

25. He was latterly engaged in composing a work on *The Nature and Effects of Order,* which he never completed, although no person was better qualified to write on the subject.

Count Rumford certainly possessed considerable facility of invention, and there was a very laudable spirit of originality in his views and modes of reasoning, although he had never leisure to acquire profound learning in any department of study. "In person he was above the middle size, with a dignified and pleasing expression of countenance, and a mildness in his manner and his tone of voice. He was ambitious of fame and distinction, and had too great a propensity to dictate," without sufficiently regarding the opinions of others who were of equal authority with himself. His mode of life was abstemious, and his health was even supposed to have suffered from too great abstinence, though his regimen was much more the result of medical opinions respecting his health, than of his own peculiar taste for temperance.

No. LXXI.

LIFE OF DR. WATSON,

Bishop of Llandaff.

Richard Watson, Bishop of Llandaff, celebrated as an able Theologian, and a Professor of Chemistry, was born in August, 1737, at Heversham near Kendal, in Westmorland. His ancestors had been farmers of their own estates for several generations, and his father had for forty years been master of the free school at Heversham, but was become infirm, and had resigned it a little before the birth of his son. The latter was, however, educated at this school, and continued there till 1754, when he was sent as a sizar to Trinity College, Cambridge. He applied without intermission to his studies, and in 1757 he obtained a scholarship, with particular expressions of approbation from Dr. Smith, who was then master. He had made it a constant practice in his mathematical pursuits, to think over the demonstration of every proposition that he studied, in his solitary walks; a habit which must certainly have been very conducive to the improvement of geometrical talent, though it could scarcely be adopted without great labour by those who follow the algebraical mode of analysis in all their investigations. After this period he passed many hours daily, for a considerable portion of his life, in the occupation of instructing others, without much enlarging the scale of his own information, though certainly not without adding to the solidity and precision of his knowledge of the most important elementary truths of science; and when he graduated in 1759, he was classed as the second wrangler, which he seems to have considered, not without reason, as the place of honour for the year, the senior wrangler, who was a Johnian, having, as it was generally believed, been unfairly preferred to him. In October, 1760, he became a fellow of

Trinity, and in November, assistant tutor of the college. Having taken his degree of M.A. in 1762, he was soon afterwards made moderator of the scholastic exercises of the university, an arduous and honourable office, which he also filled in several subsequent years.

In 1764 he undertook a journey to Paris, though without being able to speak the language, in order to take charge of his young friend and pupil, Mr. Luther, who returned to England with him soon after. He was elected in the same year Professor of Chemistry, though he had never devoted any portion of his attention to that science; but he soon rendered himself sufficiently master of all that was then known of the science, to give a very popular course of lectures on the subject about a year after his election, with the assistance of an operator whom he had brought from Paris, and to become the author of a series of essays, which served for many years as the most agreeable introduction to the elementary doctrines and the ordinary processes of chemistry. He obtained from the Government, by proper representations, a salary of 100*l.* a year for himself, and for all future professors: he paid also some attention to theoretical and practical anatomy, as having some relation to the science of chemistry. In 1767 he became one of the principal tutors of Trinity College; in 1769 he was elected a fellow of the Royal Society, and in October, 1771, he unexpectedly obtained the important and lucrative appointment of Regius Professor of Divinity, upon the premature death of Dr. Rutherford, and in that capacity he held the rectory of Somersham in Huntingdonshire. He had been little accustomed to the study of the divinity of the schools, or even of the fathers; but his eloquence and ingenuity supplied the want of theological learning, though he gave some offence to his more orthodox colleagues, by confining his arguments more strictly to the text of the Scripture than they thought perfectly consistent with the duty of a champion of the Church of England, which they considered to be the description of a professor of divinity in an English University. He attracted, however, as long as he officiated in person, audiences as numerous, to the exercises in the schools at which he presided, as had attended his chemical lectures.

He married, in December, 1773, Miss Wilson, of Dalham Tower, in Westmorland; their union continued uninterrupted for more than forty years. In 1774 he obtained a prebend of Ely, in exchange for a rectory in Wales, which the Duke of Grafton had procured for him; and he became Archdeacon of Ely in January, 1780; in the same year Bishop Keene presented him with the Rectory of Northwold in Norfolk; and, in 1782, his pupil, the Duke of Rutland, gave him the rectory of Knaptoft in Leicestershire: the same interest obtained him also from Lord Shelburne the Bishopric of Llandaff. Here his episcopal preferment rested: he generally joined the politics of the Opposition, and especially on the question of the unlimited regency: but he was too independent in his sentiments to become a very useful member of any administration; and he retired, before the end of the year 1789, without books, and with somewhat more of disgust than he ought in justice to have felt, to an estate which he had bought at Calgarth, on the banks of Winandermere, and occupied himself entirely, besides the education of his family, in agricultural improvements, especially in planting, for which he received a medal from the Society of Arts in 1789. His pupil, Mr. Luther, of Ongar in Essex, had died in 1786, and left him an estate, which he afterwards sold for something more than 20,000*l.*

He considered as one of the best practical results of his chemical studies the suggestion which he made to the Duke of Richmond, then Master of the Ordnance, respecting the preparation of charcoal for gunpowder, by burning the wood in close vessels, which, it seems, very materially improved the quality of the powder.

He had the liberality to confer, in 1804, a small living, as a reward for literary merit only, on Mr. Davies, the author of the *Celtic Researches*. The next year, he applied with success to the Duke of York for the promotion of his son, who had then the rank of a Major, and his Royal Highness speedily complied with his solicitation, as a personal favour only, without waiting for any Ministerial influence.

His health had been seriously impaired by an illness which attacked him in 1781, and which his friends attributed, though

perhaps without sufficient reason, to excessive study. In October, 1809, he had a slight paralytic affection, and another in 1811; but it was in 1813 that his last illness might be said to begin, and he sank gradually till the 4th of July, 1816. The elder of his two sons was in the army, the younger in the church: he left also several daughters. His writings are as miscellaneous as they are numerous; but none of them are bulky. The following are the principal:—

1. *Institutionum Chemicarum pars Metallurgica.* 8. Cambr. 1768. *Repr. Ess.* Vol. V.

2. Several papers in the *Philosophical Transactions* between 1770 and 1786, chiefly on chemical subjects.

3. *Apology for Christianity, in a series of Letters, addressed to Edward Gibbon, Esq.* 12. 1776. Often reprinted, and considered as very satisfactory, though the author confesses, with more of the courtier than of the orthodox divine, in a letter to Mr. Gibbon, that the *Essay* "derives its chief merit from the elegance and importance of the work it attempts to oppose."

4. *Chemical Essays,* 5 v. 12. 1781-7. Addressed to his pupil the Duke of Rutland. The work was intended for general information, and became extremely popular as a first introduction. The first volume relates to salts, sulphurs, vitriols, and gunpowder; the second to common salt, distillation, lime, clay, and pit coal; the third to bitumens, charcoal, evaporation, lead, and lead ores; and the fourth to zinc, gum, metal, tin, copper, iron, and stones; the fifth is a republication of the author's earlier chemical tracts. After the completion of these volumes, he had the resolution to burn all his chemical papers.

5. *Theological Tracts.* 6 v. 8. 1785. Collected from various authors, not excluding many works of dissenters from the Church.

6. *A Sermon on the Wisdom and Goodness of God in having made Rich and Poor*, 1785, 1793. Adapted to allay the discontents which were then prevalent among the lower classes.

7. *Apology for the Bible, in a series of Letters addressed to Thomas Paine.* 12. 1796. An able and judicious answer to

the contemptible work of a mischievous incendiary: it seems to have been singularly successful in producing clear and rapid conviction: thanks were returned to the author from Ireland and from America, and he gained 1000*l.* by the sale of the book, besides allowing it to be often reprinted gratuitously.

8. *An Address to the People of Great Britain.* 8. 1798. Enforcing the necessity of submission to the exigencies of the times. It went through fourteen editions, besides several piracies; and it was reprinted in Ireland by order of Lord Camden, then Lord Lieutenant. Mr. Wakefield answered it somewhat intemperately, and the Bishop attempted ineffectually, out of respect for his classical acquirements, to lighten the punishment which was allotted to him.

9. *A Second Defence of Revealed Religion.* 1807. In two sermons preached in the Chapel Royal.

10. *A Paper on Planting and on Waste Land.* Communications to the Board of Agriculture. Vol. VII. 4. 1808.

11. *Miscellaneous Tracts.* 2 v. 8. 1815. Religious, Political, and Agricultural. "His discourse on the first and second Adam, and the nature of death as affected by each, is almost unequalled in originality of thought, and vigour of expression." —*Quarterly Review.*

12. *Anecdotes of his Life;* revised in 1814, and published by his son, Richard Watson, LL.B., Prebendary of Llandaff and Wells. 4. Lond. 1817. *Quarterly Review,* XVIII: p. 229. Treated with great ability, but with too much severity. His chief mistake, indeed, seems to have been that he expected his literary merits alone to secure him political advancement; further than this, there is nothing disgusting, to a candid reader, in the sincerity with which he displays the consciousness of his own merits. The praises of the reviewer himself are at least as energetic as those of the friends whose language he has occasionally copied; his censures are not less impressive; but for an author's censure of himself, it would be idle to look in a work of autobiography.

He published besides a great number of sermons, charges, and pamphlets.

Though somewhat reserved, Dr. Watson is said to have been

remarkable for the simplicity of his manners and the equality of his temper. With respect to his conduct in the school of divinity, the reviewer confesses that "he ascended the chair with many eminent qualifications for his difficult and distinguished functions. The exercise of four years as moderator of the philosophical schools, had rendered his faculty of speaking Latin perfectly easy; by great assiduity the vices of his early education had been so far corrected, that a false quantity was never heard to escape him; all the tricks and shifts of school logic were familiar to his mind; in addition to which, his acuteness and ingenuity were admirable. His majestic and commanding figure, his terrific countenance, his deep sonorous voice, the uninterrupted tenor of his sentences, which, though far from classical, were never either barbarous or solœcistic, and above all, the boldness and originality of his sentiments seldom left the undergraduates' places unoccupied in the theological school. It was sport to see how the grave professor would glide over the surface of the subject with every appearance of profundity, or when pinned, as his opponent hoped, into a corner, would wind himself out with all the lubricity of an eel. Still he had a large mind; he endured, he encouraged, *he delighted in the opposition of able men; he never flinched from the strokes of those who had more information than himself*, secure in the consciousness of his own ability to encounter learning by invention. The same tolerance of contradiction, the same dexterity in parrying attacks, he brought with him into private conversation, which rendered him, when the poison of politics did not operate on his constitution, a most agreeable and amusing debater. In these happier hours, and they were not few, he would even smile at the pomp and magnificence of his own manner, and relax into all the *playfulness* and pleasantry which are almost inseparable from *real genius*."

Our critic appears, however, to have exceeded the limits of candour and of charity, when he asserts that "he was governed through life by the two leading principles of interest and ambition, both of which were thwarted, in his political conduct, by a temper so wayward, and a presumption so overweening, that the disappointment produced by their collision embittered his

mind, and exasperated his latter days to a very high degree of malignity. Accomplished as he was in academical learning, he had no ingenuous or disinterested love of knowledge: he read only that he might teach, and he taught only that he might rise."

"When he felt himself neglected, he avowedly and professedly abandoned all study, because, says he, 'eagerness in the pursuit of knowledge was a part of my temper till,' and only till, 'the acquisition of knowledge was attended with nothing but the neglect of the king and his ministers.' Disgusted, therefore, and disappointed, as much as broken in constitution, he withdrew into the wilds of Westmorland without a library, and to this privation he voluntarily submitted almost thirty years. From taste he derived neither amusement nor occupation, for of taste he never had a tincture: placed amidst the most delicious scenes of England, he thought of nothing but turning his own portion of them to emolument!" Thus, "this violent declaimer against sinecures and non-residence was the first who converted the regius professorship of divinity into a sinecure: this enemy of pluralities held at least *fourteen* places of preferment; this man of moderation in his wishes, and calm contentment, under the shade of retirement, spent the last twenty-nine years of his life in 'execrating' [complaining of] those, who, for his factious obstinacy, had left him to that retirement, while he was occupied in nursing up a fortune, till, according to his own boast, with the poorest bishopric in the kingdom, he became the richest bishop upon the bench."

With respect to the merits of the question between him and the administration of his early friend Mr. Pitt, there will probably be as many different opinions as there are readers of different political parties; but he had surely no right to expect that a ministry determined to support every minute article of the established constitution of the country, both in church and in state, should voluntarily add to the power and authority of a person who had repeatedly declared himself rather hostile than indifferent to many points which they thought essential to both; or even of one who felt so decided a conviction of the importance of every single opinion which he had himself adopted, as

to refuse his concurrence in such measures of legislation as they might deem of vital importance to the good of the country, and such as had been sanctioned by the concurrent determination of the majority of a cabinet taking on themselves the whole responsibility of their proceedings. He must have been aware that a house divided against itself cannot stand, and that the members of every administration, in a country not despotic, must consent to give up something to each other's feelings, and to make a small sacrifice of private conviction for the great objects of public energy and unanimity.

No. LXXII.

LIFE OF FOURCROY.

Antony Francis de Fourcroy, a celebrated chemist and physician, born at Paris 15th June, 1755, was the son of John Michael de Fourcroy, by his marriage with Jane Laugier. His family had been long established in the capital; several of them had been distinguished at the Bar, and Fourcroy de Ramceourt was well known as an engineer of considerable talent, and a member of the Academy of Sciences.

His father was an apothecary, attached to the household of the Duke of Orleans, and was a great sufferer by the abolition of places of this kind, which was procured by the corporation of apothecaries, some time before the revolution. Young Fourcroy was sent to the College of Harcourt, but made no progress in his learning there, and underwent great hardships from the cruelty of an unjust master. He was afterwards obliged to subsist by his labour in copying, and by taking pupils as a writing master. He was, however, fortunate in the patronage and assistance of Vicq d'Azyr, who had been a friend of his father, and under whose auspices he resolved to study physic, obtaining his support in the mean time by giving his assistance to richer persons than himself in their literary labours, and by a few translations, for which he was very ill paid. When he had gone through the regular course of study, he became a candidate for a gratuitous diploma, upon a foundation established by Dr. Diest; but he failed of success from a party quarrel. His own party, however, which was that of Vicq d'Azyr, indemnified him for the loss, by making a collection to discharge the fees, amounting to about 250*l.*; but the highest degree, that of Doctor Regent, was still refused him, and he was therefore incapable of holding a Professorship under the

Parisian Faculty of Physic. He resolved to apply himself to science as the readiest way of acquiring medical reputation, but he seems to have been little known, at any time of his life, as a practical physician. The determination, however, like that of the countryman in the fable, was still a beneficial one, and though he failed of discovering the golden treasure for which he dug, he profited by the increased fertility of the soil, and by the abundant fruits which it bore him.

In natural history he soon distinguished himself as a pupil worthy of Geoffroy, by an entomological publication; in anatomy, by his description of the tendons and their sheaths, which appears to have procured his admission into the Academy of Sciences in 1785; he stood at first in the capacity of an anatomist, though he was afterward removed to the section of chemistry. His favourite pursuit, however, from the beginning, was chemistry, and in this he derived considerable assistance from Bucquet, who was then a professor in great esteem; and having once undertaken to deliver a lecture in his place, on occasion of a temporary indisposition of Bucquet, though wholly unprepared, he found himself capable of speaking for two hours with great fluency, to the delight and astonishment of his audience. The reputation of Bucquet was soon transferred to Fourcroy, and he was enabled, by an advantageous marriage, to purchase the apparatus of his predecessor, and to succeed to his lectures.

In 1784, on the death of Macquer, then Professor of Chemistry in the Royal Garden, the Count de Buffon found the claims of Fourcroy so strong, that he thought it right to appoint him to the vacant chair, though no less a chemist than Lavoisier was a rival candidate; the competition not being wholly decided either by talent or by depth of learning, but probably, in great measure, by the reputation in the art of teaching which Fourcroy had already acquired. His success in this new situation was brilliant and universal; and he continued for twenty-five years to absorb the whole attention of a numerous audience by his eloquence, and by the perspicuity of his mode of explaining some of the most important novelties that have ever appeared in any age. The science which he taught was

then making its most rapid progress. It was then that Bergman and Scheele had introduced into analytical chemistry a precision almost geometrical; that Priestley had discovered the aeriform elements of the animal and vegetable world; that Black and Wilcke had methodised the phenomena of heat; that Cavendish had discovered the composition of water and of the nitric acid; that Monge had repeated and extended his experiments; and that Lavoisier had reduced the whole of chemistry to a uniform system, which, though founded on a generalisation somewhat too hasty, has still been of important service to the science, by concentrating the attention of the philosophic reasoner on various classes of phenomena, which could not so easily have been comprehended in one view without the aid of some such hypothesis. Mr. de Fourcroy was particularly happy in his tact of perceiving whether or no all his audience were fully in possession of the ideas he wished to communicate to them, and he was never tired of explaining himself, till he was satisfied that he had said enough. His manner was energetic, and such as an Englishman might perhaps have thought pompous and affected; but we must recollect, that there is no fixed standard of propriety in matters of taste, and that, as the common conversation of the French is naturally accompanied with more of emphasis and gesture than our own, it is very possible, that without any greater proportional exaggeration than is introduced in similar cases in Great Britain, an actor, a lecturer, or a preacher, may exhibit what to us would appear a caricature, while it only affects his own countrymen as a natural, though impressive style of public speaking. The chemical amphitheatre of the Public Garden was crowded by students from all countries, and from all quarters of the globe, some prompted to visit Paris by their own love of learning only, some assisted in their pursuits by their respective governments; and it was twice in succession necessary to provide more extensive accommodations for the overflowing numbers that sought for admittance.

Mr. de Fourcroy's political life, though not unsuccessful, seems to have contributed less materially to his happiness than his scientific career. He was chosen a supplementary member

of the National Convention, and entered on the functions of the office in the dreadful period of 1793. He had, however, the wisdom to refrain from employing the eloquence that he possessed under circumstances so dangerous, and he almost entirely confined his exertions to some attempts to soften the cruel tyranny of the times. Darcet was one of the destined victims that he had the good fortune to save; but he soon found it too dangerous to persist in such interferences. Mr. Cuvier, however, very fully acquits him of any approbation of the judicial murders that were committed, and of any connivance at such proceedings as it might have been possible for him to avert; declaring that if, upon the strictest inquiry, he could have discovered that there was the least foundation for charging him with having been indifferent to the fate of his great rival Lavoisier, no consideration on earth could have induced him to become the biographer of a person so contemptible. It was at a later period that Fourcroy acquired some little influence as a director of the public instruction; and in this capacity he had great scope for the exertion of his talents, in the re-establishment of the many public institutions connected with science, which the madness of the revolution had destroyed. The *Ecole de Médecine* was one of the first that was restored, but the name of Médecin seeming to carry with it too much of respect and authority for the levelling spirit of the day, the new institution was at first called *Ecole de Santé*. Mr. de Fourcroy was also very essentially concerned in the organization of the *Ecole Polytechnique*, as well as of the central schools of the departments, and of the *Normal* schools of Paris; nor was he an indifferent spectator of the establishment of the *Institute*, which was at first intended to be as much immediately subservient to public instruction, as to making known the results of private study. He had also considerable influence in obtaining the adoption of a law, calculated greatly to facilitate the formation of a Museum of Natural History of a magnificent extent. If, in the pursuit of these objects, he sometimes appeared to forget the dignity of language most appropriate to his subject, it must be remembered that he lived in times when the choice of expressions was by no means at the option of the speaker. He

was once denounced by the Jacobins merely for his silence in the Assembly; but he excused himself, by pleading the absolute necessity of applying himself to chemical pursuits for the support of his family.

In 1798 his duties as a senator were terminated, but he was made a counsellor of state under the consular government, and again employed in the department of public instruction, with less liberty to pursue his own ideas than before, but with more effectual means of attaining the objects of his appointment. In this capacity he directed, in the course of five years, the establishment of 12 schools of law, and of more than 30 lyceums, now called Royal Colleges, and 300 elementary schools; exhibiting in the performance of this laborious duty the greatest possible judgment and attention, in overcoming the local difficulties which perpetually occurred in the details of the undertaking, and depending on none but himself for the whole of the required arrangements: he conducted himself with great impartiality in his choice of the persons to be employed, though he sometimes found himself obliged to pay a certain degree of deference to the arbitrary power under which he acted, or even to his own political connexions. Remembering the difficulties which he had himself encountered in the early part of his career, he was particularly kind and benevolent in his intercourse with those young men to whom he was the dispenser of the public munificence, in admitting them to a gratuitous education.

The great number and extent of Mr. de Fourcroy's scientific labours may be considered as paramount to a more immediate participation in the discovery of some of the new facts which changed the aspect of the science of chemistry. His ideas were, however, often rather enlarged than profound, and he was not uncommonly somewhat too precipitate in his conclusions; but he was generally methodical in the mode of conducting his researches, and clear in relating their results. His pursuits and projects were sometimes varied a little capriciously, though he prosecuted them all with equal warmth and equal eloquence. He was too much the slave of public opinion for his own comfort, and even the slightest expression of censure that occurred

in private society, or the most unimportant criticism that appeared in a periodical work, became a heavy misfortune to him, and deprived him of his tranquillity for a considerable time. But the desire of universal approbation acted upon him as a strong incentive to continued exertion; and among all his political and his official labours, he continued his experiments, his memoirs, and his lectures, with as much eagerness as if they had constituted his whole occupation. His nerves seem ultimately to have suffered by his unremitting application, and he became subject to palpitations, which, as he was well aware, rendered the duration of his life extremely precarious. At last, on the 16th December, 1809, at the age of 54, as he was signing some dispatches, he exclaimed suddenly, "I am dead;" and his words were true. It happened, that on that day his family were about to assemble for the celebration of an anniversary, in which they were particularly interested; the assembly actually met, though only to mourn his loss: and their disappointment was rendered the greater, upon the receipt of some distinguished marks of the imperial favour, which arrived too late to be of any use to his spirits or to his health, but which would have been of the more value to him, as he had before been passed over, when some of his colleagues had received considerable gratifications. He had, however, been made a Count of the Empire and a Commander of the Legion of Honour, in addition to his various literary and scientific titles; and he must have had the heartfelt satisfaction of reflecting that he had been of use to the promotion of knowledge by his experiments and his writings; to his country by the public institutions which he had established; and to many deserving individuals by the benefits which he had bestowed on them, without the remorse of having done injury to any one.

He left a son by his first marriage with Mlle. Bettinger, the Count de Fourcroy, an officer of artillery, who was afterwards killed in the campaign of 1813, in Saxony, and a daughter, Mad. Foucaud. By his second marriage, with Mad. Belleville, the widow of M. de Wailly, he had no children. His two maiden sisters also survived him, by no means in a state of affluence, and they received great kindness from his friend and

assistant Mr. Vauquelin. His place at the Institute was very ably filled by Mr. Thenard: Mr. Laugier succeeded him at the Museum, and Mr. Gay Lussac at the *Ecole Polytechnique.*

The chief of Mr. de Fourcroy's separate publications are, 1. *Essai sur les Maladies des Artisans.* 12. Paris, 1777, translated from Ramazzini. 2. *Analyse Chimique de l'Eau Sulfureuse d'Enghien*, par Fourcroy et Laporte, 8vo. 1778, applying the recent discoveries on the nature of gases to the contents of this water. 3. *Leçons Elémentaires d'Histoire Naturelle et de Chimie*, 2 vols. 8vo. 1782, 5 vols. 8vo. 1789, 1794; afterwards translated by Nicholson. 4. *Mémoires et Observations de Chimie*, 8vo. 1784; a collection of memoirs intended as a sequel to the Elements; most of them had been read to the Academy before the author was a member, and destined for the *Mémoires des Savans Etrangers;* they relate to the metallic carbonates, to detonations, to tests for water, to combustions in a stream of oxygen, and to the properties of several saline and metallic substances, with a useful introduction on the mode of conducting chemical experiments. 5. An edition of the *Entomologia Parisiensis of Geoffroy*, 2 vols. 12mo. 1780: extracted from Geoffroy's larger work, with the addition of 250 new species. 6. *L'Art de connaître et d'employer les Médicamens dans les Maladies*, 2 vols. 8vo. 1785. 7. *Méthode de Nomenclature Chimique,* par De Morveau, Lavoisier, Berthollet, et De Fourcroy, 8vo. 1787. 8. *Essai sur le Phlogistique et les Acides*, 8vo. 1788; from the English of Kirwan. 9. *La Médecine Eclairée par les Sciences Physiques,* 4 vols. 8vo. 1791, 1792; a collection of papers, with some original essays by the editor. 10. *Philosophie Chimique, ou Vérités Fondamentales de la Chimie Moderne,* 1792, 1796, 1800. Reviewed by Deyeux, *Ann. Ch.* LVI.; a work which has been translated into almost every European language, including modern Greek. 11. *Procédés pour Extraire la Soude du Sel Marin,* 4to. 1795. 12. *Système de Connaissances Chimiques,* 10 vols. 8vo., 5 vols. 4to., 1800. This vast collection of chemical information was written from beginning to end in the space of eighteen months, during an interval of leisure from public business. *Rev. Ann. Ch.* XXXVI. XXXVII. Translated

by Nicholson. 13. *Tableaux Synoptiques de Chimie*, folio, 1800, 1805. 14. *Abrégé de Chimie, pour l'usage des Ecoles Vétérinaires.* 15. *Chimie pour les Dames*, in the *Bibliothèque des Dames.*

Besides his separate works, Mr. de Fourcroy was the author of more than 160 *Memoirs*, printed in the Memoirs of the Academy of Sciences, the Annales de Chimie, and other publications. The most important of his later researches were published jointly in his own name and in that of his pupil Vauquelin; and it is supposed that the processes were generally conducted and often suggested by Vauquelin, but that the investigations were set on foot and directed, and the results described and methodized, with inferences and theoretical reasoning, by Fourcroy.*

Mr. de Fourcroy was for some years the editor of the *Journal des Pharmaciens*. He first suggested the idea of the publication of the *Annales du Muséum d'Histoire Naturelle*, and contributed several valuable papers to it, as well as to the *Journal de l'Ecole Polytechnique*, and to the *Magasin Encyclopédique*. He was also the author of some very voluminous articles in the chemical part of the *Encyclopédie Méthodique;* but the fabric of his celebrity is principally founded on the works which have already been enumerated, and which are better known to the public.

* It has not been thought necessary to reprint the detailed list of these Memoirs which is given in the original.—*Note by the Editor.*

No. LXXIII.

LIFE OF INGENHOUSZ.

JOHN INGENHOUSZ, an eminent physician and natural philosopher, was born at Breda in 1730. He was first established in medical practice there, but removed to London in 1767, particularly with a view to study the improved methods of inoculation then lately introduced. Having become acquainted with Sir John Pringle, at that time President of the Royal Society, he was by him recommended to the Austrian ambassador, for the purpose of inoculating the Imperial family at Vienna, the Empress Maria Theresa having lost two of her children by the natural small-pox. He accepted this engagement in 1768, and having been perfectly successful in his operations, he was remunerated by the grant of a pension of 600*l*. a year for life, together with the titles of Aulic Counsellor and Physician to the Imperial Family. He was also consulted, in his medical capacity, by many others of the most distinguished personages at Vienna, and he enjoyed the particular esteem of the Emperor Joseph II., who was fond of receiving him in his cabinet, and of witnessing the exhibition of a variety of physical experiments, with which it was always the delight of Ingenhousz to amuse and instruct his acquaintance of both sexes. The following spring he went to Italy, and inoculated the Grand Duke of Tuscany. He was made a Fellow of the Royal Society in May 1769; but he appears to have remained some years in Italy; for he was at Leghorn in January 1773; in March he dates from Salzburg, and in November 1775 from Vienna. The next year he was in London; and in the winter of 1779 he went to Paris. The latter part of his life he spent principally in England, which, notwithstanding his dislike to the chilliness of the climate, was always his favourite residence, and "where he enjoyed during

many years," to use his own words, "that felicity which a free and independent man finds in the pursuit of knowledge and wisdom, in the society and friendly intercourse of those who have distinguished themselves by their learning."

Dr. Ingenhousz was cheerful in his disposition, and often playful in his conversation. Though his pursuits were chiefly scientific, he was not destitute of taste for literature and poetry. He had a particular predilection for Lucan, and for the Cardinal de Polignac, and would frequently recite passages from their poems with great energy, and with a strong German accent. Nor did he disdain the comforts of commercial opulence, and he was often a visitor at the magnificent villa of the late Mr. Rucker of Roehampton. He had been introduced there by his friend Dr. Brocklesby, who was in many respects of a perfectly congenial disposition, and who had great pleasure in prevailing on him to partake occasionally of his own hospitality, when his table would otherwise have been solitary. He died the 7th September, 1799, at Bowood, in Wiltshire, the house of the Marquis of Lansdowne, who had long known and esteemed him.

Dr. Ingenhousz's principal publications are, 1. *Experiments on the Torpedo. Phil. Trans.* 1775, p. 1. Mr. Walsh had lately gained considerable reputation by his account of the effects of the torpedo. These experiments, which were made off Leghorn, in company with Dr. Drummond, are merely illustrative of the properties of that animal, which are now better known; and they afford no decided test of the electrical nature of the phenomena.

2. *Methods of Measuring the Bulk of Mixtures of Common Air, and Nitrous Air, with Experiments on Platina. Phil. Trans.* 1776, p. 257. The eudiometrical apparatus is described as an improvement on Fontana's. The experiments are intended to show that platina is not an alloy of iron and gold, since it may be deprived of all magnetic properties by repeated cupellation.

3. *A Way of Lighting a Candle by an Electrical Spark. Phil. Trans.* 1778, p. 1022. A very small charged jar setting fire to pulverised resin, strewed on cotton.

4. *On the Electrophorus*, p. 1027. This is a Bakerian lecture, read by appointment of the President and Council of the Royal Society, relating to the instrument then lately invented by Professor Volta, and which had been made known to the author by the Archduke Ferdinand. Its action is explained upon the elementary principles of the Franklinian theory. The next article in the volume contains some experiments of Mr. Henly in confirmation of the doctrines here advanced.

5. *On a New Inflammable Gas. Phil. Trans.* 1779, p. 576. A powerful explosion is produced by the detonation of the vapour of a single drop of ether with oxygen gas. The author takes occasion to investigate the elasticity of the gas evolved by the detonation of gunpowder; and agrees with Bernoulli in estimating it as equivalent to near 2500 atmospheres. It may here be remarked, that notwithstanding Bernoulli's general accuracy and great mathematical talents, he has fallen into a very singular error, in comparing the force of gunpowder with the daily labour of men; and has accidentally made the force of one pound equivalent to the daily work of 100 men, while, in fact, the force of 40 pounds is only equivalent to the daily labour of a single man.

6. *On a Mode of Suspending Magnetical Needles*, p. 537. Proposing that a hollow needle should be immersed in linseed oil, so as to press with a small portion of its weight only on its axis, in order that the friction may be greatly diminished.

7. *Improvements in Electricity*, p. 661. On plate machines of glass and pasteboard, and on a ribbon machine. A Bakerian lecture.

8. *Experiments on Vegetables*. 8. Lond. 1779. This volume is chiefly occupied by the detailed proofs of the author's principal discovery, that vegetables in general pour out a portion of oxygen gas in the sunshine, while they rather diminish its proportion at night and in the shade. It is dedicated to Sir John Pringle, and was translated into *French* by the author. 8. Paris, 1786. Second edition, 2 vols. 8vo. 1787-9. *Latin by Scherer*. Vienna, 1786. *Dutch by Dr. Van Breda*, of Delft, with others of his works.

9. *On the Salubrity of the Air at Sea, and at Places far removed from the Sea.* *Phil. Trans.* 1780, p. 554. From the imperfection of the test employed, it was easy to imagine that some differences were discovered, which subsequent observations have shown to have no existence.

10. *Nouvelles Expériences et Observations*, 2 vols. 8vo. Paris. On different subjects of natural philosophy. In German by *Molitor*, *Vermischte Schriften.* Vienna, 1784.

11. *On the Influence of the Vegetable Kingdom in the Animal Creation.* *Phil. Trans.* 1782, p. 426. Asserting the accuracy of his experiments, and denying some statements of Dr. Priestley; advancing, in particular, many arguments to prove that the air obtained is really supplied by the vegetables, and not by the water in which they are usually immersed, in order to collect it. Dr. Ingenhousz was, on all occasions, anxious to support his claim to this very interesting discovery; and he insisted that Priestley's earlier experiments, on the green matter contained in stagnant water, had little or nothing in common with his own, because that matter was, in fact, of an animal nature. He was in the habit of collecting the gas from cabbage leaves, and of keeping it bottled up in his pocket; and he was prepared with some coils of iron wire fastened into the corks, in order to exhibit the brilliant phenomenon of their combustion to his friends: the public being at that time less accustomed to this dazzling exhibition, than it has become in later years, when elementary lectures on chemistry have been more commonly addressed to mixed audiences than heretofore.

12. *Essay on the Food of Plants*, 8vo. Lond. 1798. From the French.

13. Dr. Ingenhousz also inserted some essays in different volumes of the *Journal de Physique;* but they possess less originality and importance than his English publications.

No. LXXIV.

LIFE OF ROBISON.

John Robison, a distinguished Professor of Natural Philosophy, born, in 1739, at Boghall, in the parish of Baldernock, and in the county of Stirling, was a younger son of John Robison, Esq., who had formerly been a merchant at Glasgow, and had retired to live in considerable affluence on his estate at Boghall, not far from that city.

He was of a family sufficiently respectable to enable his son at a subsequent period to prove himself, to the satisfaction of the Court of St. Petersburg, a *gentleman* born. As a younger brother, that son was originally intended for the church, and went, at an early age, according to the custom of Scotland, in 1750, to enter as a student of humanity in the University of Glasgow; so that he was initiated almost in the rudiments of Latin and Greek literature under the able instruction of Dr. Moore, the well known professor of Greek; and he acquired such a knowledge of these languages as served to constitute him a correct classical scholar through life. He pursued his studies with so much attention, as to obtain the approbation of his teachers, and the admiration of his contemporaries, who were delighted with the originality and ingenuity of his conversation, though he did not himself reflect with perfect satisfaction upon the degree of application which he had exerted in his academical education. He took a degree of Master of Arts in 1756, having studied mathematics under Dr. Robert Simson, and moral philosophy under Dr. Adam Smith. The example of so correct and rigid a follower of the ancient methods of demonstration, as Dr. Simson, must unquestionably have exercised considerable influence on his yet unformed taste in mathematics; but he seems to have had natural preference, either from the constitution of his mind, or

from some previously acquired habits of thinking, for the geometrical method; for we are informed that "he first attracted the regard of Dr. Simson by owning his dislike of algebra, and by returning a neat geometrical solution of a problem which had been given out to the class in an algebraical form; with this mode of solution the professor was delighted, though the pupil candidly acknowledged that it had been adopted only because he could not solve the problem in the manner required of the class."

He had imbibed, in the course of his studies, an insuperable aversion to the pursuit of his original objects in the church: not certainly from any want of religious feeling, or from a dislike to the kind of life that was intended for him, but probably from some difficulties that had occurred to him respecting particular points of doctrine or of practice. He was therefore compelled to provide himself with some other occupation; and he readily accepted the offers of some of his friends in 1758, to recommend him to Dr. Blair, a prebendary of Westminster, who had formed a scheme for sending Prince Edward, the young Duke of York, to complete his professional education at sea, in company with a son of Admiral Knowles; and Mr. Robison was to have instructed his Royal Highness in mathematics and navigation. He was much disappointed, on his arrival in London, to find that the expedition had never been seriously intended; and he readily accepted an engagement to attend young Knowles as a private tutor, when he went as a midshipman on board of the Neptune of 90 guns, with Admiral Saunders, who had the command of a force intended to cooperate with General Wolfe in the reduction of Quebec; and upon the appointment of his friend as a lieutenant on board of the Royal William, Robison was himself rated as a midshipman in that ship.

The fleet arrived on the coast of America in April, 1759; in May they got up the river, and Mr. Robison was one of a party of 100 seamen draughted from the Royal William into the Admiral's ship, under the command of Lieutenant Knowles: in this capacity he had an opportunity of seeing considerable service, and of aking some surveys of the river and of the

neighbouring country, an employment for which he was perfectly qualified, both as a geometrician and as a draughtsman. He also remarked the effect of the Aurora Borealis on the compass, which had been noticed by Mairan and Wargentin some years before, but which was then not commonly known. After the battle, which was signalised by the victory and death of the gallant Wolfe, the Royal William sailed with his body to Europe, and arrived at Spithead in November. The next year she was sent to cruise off Cape Finisterre, but in six months she was obliged to return home, from having the greater part of the men disabled by the scurvy.

He used to consider the two years that he spent on board of the Royal William as the happiest of his life; and no inconsiderable part of his gratification was derived from the study of seamanship as he saw it practised under the auspices of Captain Hugh Pigot. He did not, however, acquire any firm attachment to the mode of life which he had temporarily adopted; he was rather disposed to resume his academical pursuits, and he had overcome his earlier objections to the ecclesiastical profession. He could not, however, refuse the friendly invitation of Admiral Knowles to come and live with him in the country, and to assist him in some important experiments which he was making upon mechanical and nautical subjects.

In the month of February, 1762, Lieutenant Knowles was appointed to the Peregrine sloop, of 20 guns, and Mr. Robison accompanied him with the hope of becoming a purser. He visited Lisbon and several other parts of Portugal; but he found a cruise in a small ship much less convenient and agreeable than in a large one, and, fortunately for himself and for mankind, he finally quitted the Peregrine and the naval service in June, and returned to live with Admiral Knowles; who soon after recommended him as a proper person to take charge of Harrison's timekeeper, which had been completed by the labour of 35 years, after many unsuccessful experiments, and which was now sent out by desire of the Board of Longitude to the West Indies, under the care of young Harrison and of Mr. Robison. The rate of the chronometer

was ascertained at Portsmouth the 6th of November, 1762, and it indicated, at Port Royal in Jamaica, a difference of time amounting to $5^h\ 2^m\ 47'$, which is only four seconds less than the true longitude. After a few days, the observers had a prompt opportunity of returning home by the Merlin sloop, which was sent to Europe with despatches. The voyage was most disastrous with respect to wind and weather, and at last the ship took fire; but she arrived safe at Portsmouth in March, and on the 2d of April the watch gave $11^h\ 58^m\ 6\frac{1}{2}'$, instead of 12^h, for the time of mean noon, so that the error, after six months, was only $1^m\ 53\frac{1}{2}'$, amounting to no more than about 20 miles of distance.

Mr. Robison received, upon his return, the affecting intelligence of the total loss of the Peregrine, which had foundered at sea with her commander and the whole of the ship's company. He was also greatly disappointed in the failure of some hopes which had been held out to him from the Admiralty and the Board of Longitude; though in fact there is little reason for the public to regret that he was not gratified with the pursership, which he claimed as the reward of his services. He was indeed afterwards actually made a purser by Lord Sandwich, in 1763; but he then declined accepting the appointment. His biographers very naturally complain of the neglect of those Boards which ought to have recompensed him; but certainly the Board of Longitude had no power whatever, and probably not much influence, in the appointment of a purser; and after all, the delay of a year or two was nothing very uncommon in the navy.

He had now no other resource than to return to Glasgow, and to resume his academical pursuits with renewed energy. It was from this time that he dated his serious application to his studies; he became extremely intimate with Dr. Reid and Dr. Alexander Wilson, and he had the advantage of being a witness of two of the greatest steps in the improvement of physical science that have been made in modern times, Dr. Black's experimental theory of heat, and Mr. Watt's invention of a new steam-engine. Dr. Black was the first that determined the quantity of heat required for the conversion of ice into

water; Mr. Watt, who was settled as a mathematical instrument-maker at Glasgow, had been employed in repairing a working model of Newcomen's engine for one of the professors of the university, and it was the difficulty of supplying this model with steam that suggested to Mr. Watt the eligibility of having a separate condenser, and that led him, in conjunction with Dr. Black, to a knowledge of the quantity of heat consumed in evaporation.

Amid the enthusiasm which is always inspired by the progress of scientific discovery, and of practical improvement, Mr. Robison found every encouragement and every facility for the pursuit of his favourite objects. He was recommended by Dr. Black, upon his removal to Edinburgh in 1766, as his successor in the lectureship of chemistry, though without the appointment of a professor. He took charge, also, of the education of Mr. Macdowal of Garthland, and of Mr. Charles Knowles, afterwards Sir Charles. Admiral Knowles was soon after recommended by the British government to the Empress of Russia, in order to effect a reformation in her navy, having been employed on a similar service in Portugal almost fifty years before; he had always been a firm friend to Mr. Robison, and now engaged him on this mission, with a salary of 250*l.* a year; and they proceeded together to St. Petersburg in December, 1770. Being hospitably entertained on their way by the Prince Bishop of Liege, whom they found to constitute, with his chapter and all his servants, a lodge of freemasons, Mr. Robison was easily persuaded to become one of that fraternity; in a few days he was made an apprentice, and by degrees attained the rank of Scotch master, as he has himself related in his publication upon the subject. He continued nearly two years at St. Petersburg, still acting in the capacity of private secretary to Sir Charles, who was appointed president of the Board of Admiralty, much to the advantage of the Russian navy, though his improvements were frequently retarded by the prejudices of the native officers. Mr. Robison was then appointed inspector-general of the corps of marine cadets at Cronstadt, with a double salary and with the rank of Lieutenant Colonel. His duty was to receive the report of about forty

teachers and professors, respecting the studies of 400 young noblemen, who were their pupils, and to class them according to his judgment of their merits; but he had himself nothing to teach, nor could he have had much occasion for "lecturing fluently in the Russian language," though he was introduced by his friend Kutusoff to the Grand Duke Paul as a proficient in that language; but to the Empress he was not personally known. At Petersburg he could have lived without regretting his country, in the society of such men as Euler and Æpinus, admired by the Russians, and beloved by the British; but Cronstadt in winter was deplorably melancholy; and he was induced, without much difficulty, in 1773, to make some little pecuniary sacrifice in accepting the professorship of Natural Philosophy at Edinburgh, which had become vacant by the death of Dr. Russell, and to which he had been recommended by Dr. Robertson, then Principal of the University. His determination was not disapproved by the Russian government, who granted him a pension of about 80*l.* a year for life; but it was only paid as long as three or four young men, who had accompanied him as pupils, continued to reside at Edinburgh; some discontent having been expressed because he did not keep up a correspondence with the Academy on the improvement of maritime education.

He arrived at Edinburgh in September, 1774; he married soon after, and continued to reside in that city for the remaining thirty years of his life, paying only an annual visit to his native place, where he possessed a part of his paternal estate; not being solicitous to extend it, although "he did not diminish it, otherwise than as it had been diminished before," that is, in making provision for younger children. His predecessor had been very judicious and successful as a lecturer, though not a mathematician of the highest order; he had himself more practical knowledge and experience in mechanics, and was better acquainted with the foreign mathematicians, who had naturally fallen under his notice during his residence on the Continent. His lectures were considered by most of his pupils as somewhat too difficult to be followed; a complaint which, if it did not depend on their own want of preparatory in-

formation, arose perhaps rather more from the hasty manner of his enunciation, than from the abstruseness of his matter. "The singular facility of his own apprehension," says Professor Playfair, "made him judge too favourably of the same power in others. To understand his lectures completely was, on account of the rapidity, and the uniform flow of his discourse, not a very easy task, even for men tolerably familiar with the subject. On this account his lectures were less popular than might have been expected from such a combination of talents as the author of them possessed." This inconvenience was increased "by the small number of experiments he introduced, and a view that he took of Natural Philosophy, which left but a very subordinate place for them to occupy. An experiment, he would very truly observe, does not establish a general proposition, and never can do more than prove a particular fact:" but he seems to have carried this principle to some little excess: it is, in fact, the *illustration*, and not the *proof*, of general principles that is the object of a public exhibition of experiments; and it is very doubtful whether Archimedes, or Newton, or Leibnitz, or Euler, would have been very successful as *showmen*. With respect, however, to "accuracy of definition, to clearness, brevity, and elegance of demonstration, and even to neatness and precision in experiments," Professor Robison *was* very successful; his course extended "to every branch of physics and of mixed mathematics," and entered so fully into the detail of each particular division of the subjects, that "a more perfect system of academical instruction is not easily to be imagined:" nothing, in short, was wanting, but so much previous knowledge of mathematics in his pupils as he thought he had a right to expect, though his expectations were too rarely fulfilled.

The Philosophical Society of Edinburgh had been almost suffered to sink into oblivion after the publication of the third volume of its *Essays*, in 1756. Professor Robison became a member of it soon after his return from Russia, and was chosen Secretary of the new Society upon its formation by Royal charter, in 1783, when it incorporated with itself the whole of the surviving members of the former Society. In 1798, he received the compliment of a degree of Doctor of Laws from

the University of New Jersey; and a similar honour was paid him at Glasgow the year after. In 1800 he was elected, as successor to Dr. Black, on the list of the Foreign Members of the Royal Academy of Sciences of St. Petersburg.

He was attacked, in 1785, by a severe disorder, from which he was never afterwards wholly free, though it produced little inconvenience, besides pain, with some depression of spirits, which was, however, attributed rather to the closeness of his application than to the immediate effect of the disease, which was a glandular induration. For many years he was obliged to obtain the assistance of substitutes in the delivery of his lectures; but towards the end of his life he was able again to perform the duties of the professorship in person. He continued his literary labours with little intermission, and was most happy in the care and attention of his wife and children, whose virtues he found the best alleviation of his sufferings. He took a slight cold, after giving a lecture, on the 28th of January, 1805, and died on the 30th.

1. It was comparatively late in life that Professor Robison assumed the character of an author, having communicated to the Royal Society of Edinburgh, in 1785, a paper on the *Determination of the Orbit and Motion of the Georgium Sidus*, which was published in the *Edinburgh Transactions*, Vol. I. He had observed the opposition of the planet in 1786, with an equatorial telescope only, and he had computed the elements of its orbit with greater accuracy than any other astronomer had then done; although his suspicion of the effect of such a planet on the motions of Jupiter and Saturn has not been confirmed by later investigations, the irregularities of these planets, on the contrary, having been otherwise explained.

2. A second paper, published in the same collection, Vol. II. p. 82, relates to *The Motion of Light*, as affected by refracting or reflecting substances which are themselves in motion: the author corrects some errors of Boscovich, who had miscalculated the effect of a water telescope, but he seems to agree with Dr. Wilson in the suggestion of another experiment of a similar nature, which, to say the least, is wholly superfluous.

3. The most important, beyond all comparison, of Professor Robison's scientific publications, are the articles which he communicated from time to time to the *third* edition of the *Encyclopædia Britannica*, and to its *Supplement*. It was under the care of Mr. Colin Macfarquhar that the first twelve volumes of that edition of this work were published;* and upon his death, in 1793, the task of continuing it was committed to Dr. Gleig, to whom Professor Robison became a most essential co-operator, and from that time "the work ceased to be a mere compilation." The first of his contributions, according to Professor Playfair, was the Article OPTICS, but he probably only revised and enlarged that article; it was followed by PHILOSOPHY, which he wrote jointly with Dr. Gleig; by PHYSICS, PNEUMATICS, PRECESSION, PROJECTILES, PUMPS, RESISTANCE, RIVERS, ROOF, ROPE-MAKING, ROTATION, SEAMANSHIP, SIGNAL, SOUND, SPECIFIC GRAVITY, STATICS, STEAM, STEAM ENGINE, STEELYARD, STRENGTH, TELESCOPE, TIDE, TRUMPET, VARIATION, and WATERWORKS; and in the *Supplement*, by ARCH, ASTRONOMY, BOSCOVICH, CARPENTRY, CENTRE, DYNAMICS, ELECTRICITY, IMPULSION, INVOLUTION, MACHINERY, MAGNETISM, MECHANICS, PERCUSSION, PIANO-FORTE, POSITION, TEMPERAMENT, THUNDER, TRUMPET, TSCHIRNHAUS, and WATCHWORK. Notwithstanding some degree of prolixity and want of arrangement, which could scarcely be avoided in the preparation of original articles for such a mode of publication, the whole of them, taken together, undeniably exhibit a more complete view of the modern improvements of physical science than had ever before been in the possession of the British public; and display such a combination of acquired knowledge with original power of reasoning, as has fallen to the lot of a few only of the most favoured of mankind.

4. It is not altogether with so high approbation that his friends and his biographers have mentioned a work, of a nature rather political than philosophical, entitled *Proofs of a Conspiracy against all the Religions and Governments of Europe,*

* The third edition of the 'Encyclopædia Britannica' consisted of eighteen volumes; the Supplement to it of two volumes.

8vo. Ed. 1797; though it went through several editions. The principal part of the book consists of the history of the *Illuminati* and the *German Union*, whom he considers as having become the chief agents in a plot first formed by the Freemasons, at the suggestion of some ex-jesuit, who proposed for their model the internal economy of the order which he had quitted; and whatever foundation this outline may have had in truth, there is no doubt that the manner, in which Professor Robison has filled it up, betrays a degree of credulity, extremely remarkable in a person used to calm reasoning and philosophical demonstration. For example, in the admission of a story told by an anonymous German author, that the minister Turgot was the protector of a society that met at Baron d'Holbach's, for the purpose of examining the brains of *living children*, in order to discover the principle of vitality. He does not accuse the English Freemasons of having participated in the conspiracy; but he considers the Continental lodges as having been universally implicated in it.

5. After the death of Dr. Black in 1799, he undertook to superintend the publication of his *Lectures on Chemistry*, which appeared in 1803, 2 vols. 4. And he executed this task, which was rather laborious than difficult, with equal zeal and ability. He endeavoured to reduce to their just estimate the comparative pretensions of the French and British chemists, though he is somewhat puritanically severe in criticising the literal meaning of the compliments paid to Black by Lavoisier, on which he founds a charge of insincerity.

6. His last publication was the first volume of a series which was to form a complete system, entitled *Elements of Mechanical Philosophy*, 8. Ed. 1804. It comprehended only *Dynamics* and *Astronomy*; and it never became very popular; it was too difficult for the many, who wished for general and philosophical notions only, and not sufficiently precise and demonstrative for the few, who wanted practical and numerical results. In attempting to combine the separate merits of the *Exposition du Système du Monde*, and of the *Mécanique Céleste*, the author sacrificed both the popular simplicity of the former, and the mathematical perfection of the latter. A few inaccuracies

which ought not to escape the attention of the reader of the work, have been pointed out in the *Imperial Review* for March, 1805, (p. 263);* a journal long since discontinued.

7. The contents of the volume last mentioned, together with some manuscripts intended to have formed part of a second, and the greater part of the articles furnished by Professor Robison to the *Encyclopædia*, were collected into a *System of Mechanical Philosophy, with Notes*, by David Brewster, LL.D. 4 vols. 8, Ed. 1822; a spirited bookseller in London having undertaken the risk of the publication. 1. The first volume begins with the articles DYNAMICS, PROJECTILES, CORPUSCULAR FORCES, CAPILLARY ATTRACTION, BOSCOVICH'S THEORY, and ROTATION, all as remodelled for the *Elements*: then follow STRENGTH OF MATERIALS, CARPENTRY, ROOF, CONSTRUCTION OF ARCHES, CONSTRUCTION OF CENTRES. 2. The article STEAM-ENGINE is enriched with notes and an appendix, by the late Mr. Watt; the next is MACHINERY; then RESISTANCE OF FLUIDS, RIVERS, WATERWORKS, PUMPS. 3. ASTRONOMY, TELESCOPE, PNEUMATICS. 4. ELECTRICITY, MAGNETISM, VARIATION, TEMPERAMENT, TRUMPET, WATCHWORK, and SEAMANSHIP. The notes are not numerous, and the editor's principal labour has been to retrench some passages that appeared to him superfluous, when the papers were to stand as parts of such a collection.

"Although Dr. Robison's name," says Dr. Brewster in his preface, "cannot be associated with the great discoveries of the century which he adorned, yet the memory of his talents and his virtues will be long cherished by his country. Imbued with the genuine spirit of the philosophy which he taught, he was one of the warmest patrons of genius wherever it was found. His mind was nobly elevated above the mean jealousies of rival ambition, and his love of science and of justice was too ardent to allow him either to depreciate the labours of others, or to transfer them to himself. To these great qualities as a philosopher, Dr. Robison added all the more estimable endowments of domestic and of social life. His friendship was at all times generous and sincere. His piety was ardent and unosten-

* This article was written by Dr. Young. – *Note by the Editor.*

tatious. His patriotism was of the most pure and exalted character; and like the immortal Newton, whose memory he cherished with a peculiar reverence, he was pre-eminently entitled to the high distinction of a Christian, patriot, and philosopher." His person was handsome, and his physiognomy prepossessing; and he appears to have been endowed with an extraordinary combination of talents, even exclusively of those which were called into immediate activity in his professional pursuits; for he was a good linguist, an excellent draughtsman, and an accomplished musician: his conversation was always energetic and interesting, and sometimes even poetical; and his liberality of sentiment was only limited by his regard for what he considered as the best interests of mankind.

A short account of his life was published in 1802, by a contributor to the *Philosophical Magazine*, who, among other inaccuracies, thought himself at liberty to assert that he was an admirer of the algebraical form of representation, in preference to the geometrical. His friend, Dr. Gleig, stepped forwards soon after to correct these mistakes, in the *Antijacobin Review* for 1802, and his letter was copied into the *Philosophical Magazine*. He asserts, from his own knowledge, that even yet Professor Robison "delights much more in geometry than in any of the modes of algebra, assigning, as the reason of his preference, that in the longest demonstration, the geometrician has always clear and adequate ideas, which the most expert algebraist can very seldom have." It may perhaps be asserted, on the other hand, that the same reasoning would lead us always to employ actual multiplication or division, in preference to the use of logarithms or of a sliding rule; and that the whole of the magic of calculation depends on the abstraction of the results from the numerous and separately unimportant steps by which they are obtained: but the having once seen those steps clearly is certainly of great importance to the process of reasoning, even when the memory no longer retains them; and no mathematician of correct taste can study the ancient geometricians without admiring the elegance and precision of their method, even amidst the pedantry which too frequently envelopes their expressions, and without being grateful for their

punctuality in collecting their results into the very convenient form of distinct propositions, and in making such references from each proposition to the foundations on which it depends, as to enable him readily to trace back their steps to the most elementary principles; which is scarcely possible in any of the works of the most modern school of analysis. Professor Robison, however, seems rarely to have cultivated the higher mathematics for their own sake only, or any further than as they could be applied to the study of the phenomena of nature, or to the practice of the combinations of art; in fact, without some such limitation, there would be no track to guide us in the pathless regions of quantity and number, and their endless relations and functions. But besides the utility of the pure mathematics, as a branch of early education, in exercising and fortifying the powers of the mind, it is impossible to foresee with certainty *how much* of mathematics may be wanted by the natural philosopher in any given investigation; and Professor Robison, as well as many others of his countrymen, would certainly have been the better for the possession of *a little more*, as the author of the criticisms in the *Imperial Review* has already had occasion to remark.

No. LXXV.

LIFE OF DOLOMIEU.

DEODATUS GUY SILVANUS TANCRED DE GRATET DE DOLOMIEU, a distinguished mineralogist and geologist; son of Francis de Gratet de Dolomieu, and Frances de Berenger, was born the 24th of June, 1750, in the province of Dauphiné.

He was admitted a member of the order of Malta during his earliest infancy, as if he had been devoted from his cradle to glory and to misfortune. At eighteen he embarked in one of the galleys belonging to the order, and soon after unhappily found himself under the necessity of fighting a duel, in which his adversary fell. The laws condemned him to die; but he received a pardon from the grand-master; it was, however, necessary that it should be approved by the Pope, who for a long time refused to confirm it, notwithstanding the solicitations of several European powers in behalf of the offender; until his consent was at last obtained by the cardinal Torregiani. Dolomieu in the mean time was closely imprisoned in the island for nine months, and this period of solitude seems to have contributed materially to increase the seriousness of his character, and to confirm him in a contemplative turn of mind.

At the age of twenty-two he went to Metz, as an officer in the regiment of carabiniers, in which he had held a commission for seven years; and he displayed great courage and personal activity on occasion of an accidental conflagration, which occurred soon after. His leisure hours were employed in the study of chemistry and natural history, with the assistance of Mr. Thirion, an apothecary residing in this city. He also became intimate about the same time with De la Rochefoucault, with whom he maintained an unshaken friendship ever after.

1. He commenced his literary career with an *Italian Translation of* Bergman's *Work on Volcanic Substances*, to which he

added some *notes*, and some observations on the *classification* of those substances.

2. He also furnished some *notes* to a translation of Cronstedt's Mineralogy.

3. In 1775 he published *Researches on the Weight of Bodies* at different distances from the earth's centre; and upon the recommendation of La Rochefoucault, was made a correspondent of the Academy of Sciences at Paris. This compliment seems to have contributed to his determination to relinquish his prospects of success in the army, and to devote himself exclusively to science. Having resigned his commission, he commenced his geological labours with a tour in Sicily, Italy, and Switzerland.

4. This expedition afforded him the materials for his *Voyage aux Iles de Lipari fait en* 1781, which he published in 1783 with some other tracts. He describes a singular kind of volcano at Macaluba in Sicily, formed by air bubbling up from the crater, and causing its contents to overflow. The Essay on the Climate of Malta is rendered inconclusive by the imperfection of the eudiometrical apparatus, that was then commonly employed.

5. He spent a part of the same year in examining the effects of the earthquake in Calabria, which are described in his *Mémoire sur les tremblemens de terre de la Calabrie.* 8vo. Rome, 1784. Among other observations he notices the singular fact, that all those parts of Calabria, to which the earthquake extended, are of a calcareous nature, without any traces of volcanic substances.

6. He published in the *Journal de Physique,* Vol. XXV. p. 191, a paper *on the extinct volcanoes* of the Val di Noto in Sicily.

7. His *Mémoires sur les Iles Ponces,* 8, 1788, contains also a catalogue of the productions of Mount Etna, and an account of the eruption of 1787.

At the beginning of the revolution, Dolomieu embarked, together with his friend La Rochefoucault, in that which appeared to be the cause of liberty. He was in Paris on the 14th of July, but he did not accept of any office under the newly

modified government. La Rochefoucault soon fell a victim to the horrors of the times. Dolomieu was present in his last moments, and received the affectionate messages which he sent to his mother and his wife, who were more distant witnesses of the dreadful scene.

8. No longer hoping for any benefit to his country from the political events of the day, he appears to have resumed his geological studies in other parts of Europe. In a *Letter on the origin of basalt*, dated Rome, 1790, *Journ. Phys* Vol. XXXVII. p. 193, he considers some stones of this description, for instance, the black trapps of Saxony, as the productions of water; and others, particularly the varieties found in the south of Europe, as of volcanic origin.

9. He writes, in 1791, a *Letter from Malta*, describing a species of limestone, found in the Tyrol, hard enough to become phosphorescent upon collision, and not effervescing with acids until powdered. It was afterwards called the Dolomite. *Jour. Phys.* Vol. XXXIX. p. 3.

10. In a paper of *Directions for Naturalists*, he gives some useful advice to the circumnavigators about to sail to the South Seas. *Jour. Phys* Vol. XXXIX. p. 310.

11. A series of his essays *On Compound Stones and Rocks* appeared from time to time in the *Journal de Physique*, Vol. XXXIX p. 374; Vol. XL. p. 41, 203, 372. In these he insists on the necessity of supposing that the ocean must have acted with great violence, in reducing the continents into their present state; neither the slow subsidence of a general deluge, nor the continued action of ordinary rivers, being sufficient to explain the phenomena; and he remarks, that a violent agitation, such as must necessarily be supposed to have taken place, would naturally cause several alternations in the state of the waters, like immense waves or tides, which must have contributed to the modifications impressed on the earth's form. Indeed, the facts which support this opinion appear to be so obvious and so numerous, that it is difficult to understand how the opposite hypothesis could ever have become popular.

12. In the same volume there is a short paper *On Petroleum found in Rock-Crystal*, and on some elastic fluids obtained from it, p. 318.

13. The progress of his memoirs was now interrupted by the proscription, in which many of the best and wisest of his countrymen were indiscriminately involved. "His duty and his inclination," he says in a *Note* without a date, "required the devotion of his time and his arm to the defence of his king;" and he was obliged to submit to a temporary dereliction of his pursuits of science. P. 481.

14. But the cause was hopeless; and it was impossible for him to render it any essential service. He soon resumed his pen, and took occasion to express, with great spirit and energy, his political feelings, in his *Memoir on the Physical Constitution of Egypt. Jour. Phys.* Vol. XLII. p. 41, 108, 194. In Egypt, he observes, there are many calcareous rocks, and sands, which cannot have been brought down by the Nile; but there is also much of the soil which has the appearance of having been derived from the mud, with an admixture of sand only. The same cause, he thinks, may possibly have raised the bed of the river, so that the relative height of the inundations may have been little altered. He conceives that the Delta has increased even in modern times, though far less rapidly than it appears to have done formerly; for he is disposed to admit the credibility of the Homeric account of the distance of the Pharos from the continent, although he attempts to explain a part of the supposed change, by the filling up of the lake Mareotis only; and, on the whole, he imagines that about 1000 square leagues of the surface of Egypt have been gained from the sea. He has not, however, thought it necessary to discuss the arguments, which Bruce and others have brought against the established opinion, and against the facts asserted by Herodotus in its support; although some of the best informed of modern travellers have allowed the accuracy of Bruce's statements relating to this subject.

15. In a short paper *On the Figured Stones of Florence*, Mr. Dolomieu attributes the appearance of the arborescent and architectural figures, which characterize them, to the process of slow decomposition and oxydation, gradually producing the stains in the extremely minute fissures, which favour these changes. *Jour. Phys.* Vol. XLIII. p. 285.

16. Upon the establishment of the School of Mines, in 1795, he accepted the situations of Professor of Geology and Inspector of Mines. He was also made one of the original members of the National Institute of Sciences and Arts, then organized by a law of the existing government. From this time he appears to have redoubled the energy with which he had before laboured in the pursuit of natural knowledge, and he published a great number of memoirs in the course of a very few years. One of the first of these consisted of *Observations on a pretended Coal Mine, called the Désirée. Journal des Mines*, Year III. N. ix. p. 45.

17. His *Methodical Distribution of Volcanic Substances* appeared in the new *Journal de Physique*, Vol. (I.) XLIII. p. 102, 175, 241, 406; Vol. (II.) XLIV. p. 81. Of the five classes, which he had before proposed in his notes on Bergman, the first comprehends substances actually produced by volcanos; the second, substances thrown out by them unaltered; the third, bodies altered by the volcanic vapours; the fourth, bodies altered in the moist way; and the last, substances illustrative of the history of volcanos only. The subsequent papers are partly continuations of the *Memoirs on Compound Rocks;* and they also relate particularly to the nature of lavas, some of which are shown to be formed from argillaceo-ferrugineous stones. The heat of lavas has been pretty accurately ascertained, in some cases, by the fusion of silver coins exposed to it, while those of copper remained entire; there is, however, an account of a stream of lava over which some nuns are stated to have walked while it was yet fluid; and this circumstance Mr. Dolomieu attributes to a mixture of sulphur, which remained melted at a temperature comparatively low. Some objections to this opinion have, however, been advanced by Mr. Sage. *Journ. Phys.* Vol. XLV. p. 281. *An Explanation of the New Method adopted in the Description of Minerals*, was also published in the *Magasin Encyclopédique*, Vol. I. p. 35.

18. Among the shorter essays of Mr. Dolomieu, we find a *Description of the Beryl*, *Jour. des Mines*, year IV. Ventose, p. 11.—19. *Description of the Mine of Manganese at Romanèche.* Germinal, p. 27.—20. *Letter on the Heat of Lavas.* Messidor,

p. 21.—35. *On Quartzose Concretions*, p. 56.—22. *On Ancient Lithology. Mag. Enc.* I. p. 437.—23. *Description of the Emerald*, II. p. 17, 145.—24. *A Letter from Berlin on the Magnetic Serpentine*, II. Vol. VI. p. 7.—25. *On the Leucite, or White Garnet. Journ. des Mines*, year V. p. 177.—26. *On the Necessity of Chemical Knowledge to a Mineralogist; and on the term Chrysolith*, p. 365.

27. *An Introductory Discourse on the study of Geology* appears in the *Journal de Physique*, Vol. XLV. p. 256. It was preliminary to a course of lectures on the natural position of minerals; and it contains good and detailed directions for the use of students, with some eloquent advice on the benefits of travelling, and on the merits of temperance and simplicity of manners.

28. In the next volume, p. 203, our author announces the *Discovery of the Crystallized Sulphate of Strontia in Sicily*. It had before been found uncrystallized in France.

29. *On Colour as a Characteristic of Stones. Jour. Phys.* Vol. (III.) XLVI. p. 302. This essay contains some objections to Werner's habit of relying too implicitly on colour; and the white tourmaline of St. Gothard is adduced as an instance of the triumph of form over complexion: a just tribute of commendation is also paid to the merits of Haüy.

30. A paper *On the Pyroxene, or Volcanic schörl*, is chiefly destined to support the opinion that such crystals have been formed previously to the existence of the volcano, by the observation of a specimen found adhering to a rock which had never undergone the effect of fire. *Journ. Phys.* Vol. XLVI. p. 306.

31. *A Memoir read to the Institute* contains the report of Mr. Dolomieu's mineralogical tours made in the years 1797 and 1798. *Journ. Phys.* Vol. XLVI. p. 401. *Journ. des Mines*, year VI. p. 385. He visited the south of France, the Alps and the neighbouring lakes and mountains, almost always on foot, and with his hammer in his hand, accompanied by Brochart, Cordier, Bonniers, and his brother-in-law, the Marquis de Drée. From his observations in Auvergne, in particular, he concludes that the foundation or origin of the volcanos

there is certainly below the granite rocks, which therefore cannot, properly speaking, be called primitive; and he proceeds to a much bolder and less admissible conjecture, that the central parts of the globe are at present in a state approaching to fluidity, which he attempts to support by the ready transmission of the shocks of earthquakes to distant places; and he even quotes the authority of Lagrange as having been disposed to encourage the opinion. Volcanos, in general, he divides into ancient and modern, as separated by the intervention of the changes which have reduced the continents to their present form. With respect to the heat of the lava, he observes that it has not been sufficient to expel the carbonic acid from the limestone which has been exposed to it. He also remarks, that, where basalt in fusion has been suddenly cooled by water, the contraction has caused it to divide into columns, which are not crystalline, because their angles are irregular, and which are smaller and more uniform in proportion as the water is deeper. He contrasts the horizontal strata of France with the vertical tables of the Alps; and particularly describes the accretion of a mantle of calcareous substances, two miles in height, which has attached itself to the north-east faces of the Alps, subsequently to their first formation as mountains. From this expedition he brought home an immense collection of rocks and stones, principally valuable for their arrangement with a view to the illustration of his particular doctrines in geology; which, with the rest of his cabinet, have since formed a part of the superb museum of Mr. de Drée.

32. He published, about the same time, a paper *On the Mountains of the Vosges. Journ. des Mines*, year VI. p. 315.

33. *Extract of a Report on the Mines of the department of the Lozère*, p. 577.

34. The only communication of Mr. Dolomieu, printed in the *Memoirs of the Institute*, is rather on a mechanical than a mineralogical subject, containing an *Account of the art of making Gun-Flints. M. Math.* Vol. III. p. 348. Nicholson's *Journal*, 8. Vol. I. p. 88.

He was engaged, after his return from Switzerland, in some mineralogical contributions to the *Encyclopédie Méthodique;*

when he was invited to take a part in the scientific arrangements of the expedition to Egypt. He did not, however, strictly confine himself to this department; but was successfully employed as a negociator for the surrender of Malta. In Egypt he visited the pyramids, and examined some of the mountains which form the limits of the country; but his health soon compelled him to return to Europe. In this voyage the vessel on board of which he had embarked was nearly overwhelmed by a tempest, and appears to have been only saved by the temporary expedient of throwing overboard pounded biscuit mixed with straw, which entered the leaks with the water, and afforded a partial remedy, which was repeated from time to time, until the vessel, at the last extremity, was driven into a port in the Gulf of Tarentum. The counter-revolution of Calabria had occurred but a few days before; and Dolomieu, with his companion Cordier, and many others of his countrymen, were thrown into prison; and they even owed their lives to the great exertions of an individual among the insurgents in their favour. They were afterwards removed to Sicily, but with the loss of their collections and their manuscripts; and Dolomieu, being denounced as a member of the order of Malta, for high treason, was separated from his countrymen, and closely confined in a dungeon. Solicitations were addressed to the King of Naples, on his behalf, by the National Institute, by the French government, by the King of Spain, and in the name of the Royal Society of London, although its illustrious President was certainly not "at the time in Sicily," as the *Nouveau Dictionnaire Historique* affirms; but the captive derived essential assistance from the good offices of an English gentleman at Messina, and some Danes accommodated him in his pecuniary arrangements. While still a prisoner, he was appointed successor to Daubenton, at the Museum of Natural History; and the very circumstance of his captivity seemed to give him an advantage over his competitor. In the treaty made by the French with the King of Naples, after the battle of Marengo, it was expressly stipulated, that Dolomieu should be set at liberty.

35. Upon his return to Paris, he was made a member of

the Conservative Senate, and he delivered soon after a course of lectures on the philosophy of mineralogy. He had written part of an essay on this subject during his imprisonment in Sicily, with a bone for a pen, and a mixture of soot and water instead of ink, on the margins of such books as were allowed him; and his last publication was *Sur la Philosophie Minéralogique et sur l'espèce Minéralogique.* Paris, 1801. His classification depended on considering the species as determined by the integrant molecule, and on arranging the different external forms as varieties, whether regular, as modifications, or irregular, as imperfections; besides the variations of colour and appearance, and the more essential affections of the consistence of the substance, which may be called contaminations; but the whole essay may be considered as rather of a logical than of a physical nature.

After the delivery of his lectures, he set out upon a new expedition to his favourite mountains, in company with Mr. Neergard and Mr. d'Eymar, who published an account of the journey. 8. Par. 1802. He meditated a tour into Germany and to the North of Europe, but his return to Paris was interrupted by indisposition, when he had arrived, by way of Lyons, at Châteauneuf, where he met his sister and his brother-in-law; and this journey was his last.

The merits of Dolomieu consisted as much in his personal character, as in his scientific attainments. His conversation was modest, though his courage was heroic; his manners were simple though refined; and though his talents were considerable, they seem to have been surpassed by his industry. It has been remarked, that he often undertook more than he had any reasonable prospect of completing; but, in the mean time, he was perhaps as happy in the pursuit, as he would have been in the attainment of his object. He died, universally regretted, at Drée, near Macon, the 27th of November, 1801, in the midst of his affectionate family, who had been the partakers in his pursuits, and the consolations of his misfortunes.

No. LXXVI.

LIFE OF COULOMB.

Charles Augustin Coulomb, a profound and ingenious theoretical mechanic and natural philosopher, descended from a distinguished family of Montpellier, was born at Angoulême, the 14th of June, 1736. He felt, at an early period of his life, a strong preference for mathematical studies, and would gladly have devoted his whole attention to the pursuit of science; but he found it more convenient to enter the military profession as an engineer. This department, however, afforded him ample scope for the exercise of his powers of observation and calculation: and after having been ordered on service to America, and remaining abroad nine years, with some injury to his health, he presented to the Academy of Sciences, in 1773, a memoir on cohesion, and on the resistance of various works of masonry, which, for the accuracy and originality of the views that it exhibits, for the clearness and neatness of the demonstrations, and for the practical utility of the results, is fully equal to any of his later productions, and shows a mind still in the vigour of youth, and yet matured by the approach of middle age. The Academy paid him the compliment of making him one of its correspondents, and in 1779 he had the satisfaction of sharing, with the laborious Van Swinden, the prize proposed for improvements in the construction of compasses. He resided for some time at Rochefort, where he had abundant opportunity of prosecuting, in the naval arsenal, his experimental researches on friction, which obtained him in 1781 the double prize for the theory of the effects of simple machines: in the same year he had the good fortune to be stationed permanently at Paris, and becoming a member of the Academy, devoted himself entirely to the investigation of the

laws of electricity and magnetism, and of the force of frictions and resistances of various kinds. He is generally supposed to have been the first that proved, by direct experiments, the law of the decrease of electrical and magnetic forces in the proportion of the squares of the distances: but it must not be forgotten that the late Lord Stanhope had published an experiment, five years before the date of Mr. Coulomb's researches, which sufficiently established this law with respect to electricity; although the extension of the same law to the operation of magnetism appears to belong exclusively to Mr. Coulomb. He continued to occupy himself in these researches till the time of the revolution, when he was expelled from Paris by the decree that banished all the nobility; having before given up the appointment of Intendant general of fountains, and having otherwise very materially suffered in his property. He retired with his friend Borda to a small estate which he possessed at Blois; and during his residence there, made some observations on vegetable physiology, which he afterwards presented to the Institute.

He was recalled to Paris in order to take a part in the new determination of weights and measures, which had been decreed by the revolutionary government. He returned, soon after, into the country, wishing to devote himself to the care of his family, and of the remains of his little fortune. But upon the establishment of the National Institute, he again became an inhabitant of the metropolis. He had, however, occasion to undertake a tour of considerable extent, in discharging the duty of an inspector of public instruction; and he was remarked, in his examinations of the young students, for the singular good nature and paternal tenderness of his manners. He still continued his application to his favourite pursuits, and in particular to the investigation of the magnitude of forces of various kinds, by means of the principles of torsion. And his last study was an inquiry respecting the universal diffusion of the magnetic power through nature, which he at first supposed to be almost unlimited; although he afterwards found reason to conclude, that its general cause was the presence of a minute quantity of iron. A short summary of his numerous

and elaborate memoirs will best illustrate the extent and accuracy of his researches.

1. *Statical Problems relating to Architecture. Mém. Sav. Etr.* VII. 1773. p. 343. The fluxional modes of ascertaining maxima and minima are applied, in this admirable memoir, to the determination of the strength of blocks of stone and of pillars of masonry, and to that of the resistance of semi-fluids, and the thrust of earth. The author's manner of considering the subject was at that time new; it has been still further extended by some late researches in this country (*Hutton's Mathematical Dictionary, Art. Pressure*);* and many of the calculations contained in the articles BRIDGE and CARPENTRY† of this *Supplement*, are principally founded on the same basis. Mr. Coulomb very properly objects to Musschenbroek's mode of representing columns as exerting their passive strength like bars resisting flexure; and it is surprising that Mr. Lagrange did not profit by his remarks, in abridging his laborious investigations of their elastic force. Mr. Coulomb appreciates the friction of soft materials by the angle at which they will stand unsupported: an angle which has been termed in this country the angle of repose, by an author who perhaps imagined there was more novelty in the idea, than he would have done, if this memoir had been fresh in his mind. For the thickness of the walls of an embankment, one-seventh of the height is recommended as a good proportion in common cases, with an increase of one-sixth towards the bottom: But the calculation is left in some measure incomplete, although it may be sufficiently accurate for the cases which most commonly occur in practice. In the last place, the proper direction of the joints of flat arches is determined; and the point of easiest fracture of arches in general is investigated, by an approximatory method, applied successively to each joint.

2. *On the improvement of the construction of the Compass. Mém. Sav. Etr.* IX. p. 165. This memoir contains a laborious and accurate investigation both of the operation of the force of magnetism, and of the resistances exhibited to the motion of a needle by friction or by other causes. The author lays down,

* No. XLIX. in this volume. † Nos. LII. and LIII. in this volume.

as a fundamental principle, the quality of the accelerative force acting on a given needle, in all positions, when referred to the direction of the meridian; so that the properties of its vibrations become precisely similar to those of a pendulum actuated by the force of gravity. From the existence of an attractive and a repulsive force at the same time in each particle of the body, with respect to the opposite magnetical poles of the earth, he concludes the total impossibility of explaining the effects by means of any ethereal currents or vortices which had been admitted by some of the best mathematicians of the century, then still surviving. He examines the comparative force of needles of different dimensions, and proves the extreme delicacy of the suspension afforded by a long fibre, whether of hair or silk; and he describes a compass in which the reading is performed by means of a microscope fixed to a graduated arm, serving as a vernier. In order to find the true direction of the magnetic action of a needle, he turns it upside down, and takes the mean of the observations. He ascertains the magnitude of the friction of steel on glass, by measuring the angle of repose; and he finds that it is equal to $\frac{2}{11}$ of the pressure. He then proceeds to calculate the friction of a pivot, supposing it to be compressed most at the centre, but concludes, from a hasty experiment, that the magnitude of the surface is not much changed by a change of pressure. He infers, however, that a light needle has an advantage over a heavy one. A perforation in the middle of a needle appears to interfere but little with its magnetic force. He proposes to ascertain the position of the dipping needle, by measuring the frequency of the horizontal vibrations, and the force required to keep it level: and he concludes his memoir with an account of the diurnal variations of the needle, and a conjecture respecting the operation of the sun and his atmosphere, which he compares to a large aurora borealis; referring the secular change of the variation to the slow motion of the sun's apogee.

3. *Recherches sur les moyens d'exécuter dans l'eau toutes sortes de travaux hydrauliques.* 8. Par. 1779. Rozier XIV. P. 393. *Addition.* Rozier XVII. P. 301. Ed. 2. 8. Par. 1797. The Academy of Rouen had proposed a prize for an essay on

the best mode of lowering a rock in the Seine at Quillebœuf, which was about a foot below low-water mark: this essay was originally written for the prize, but it was published without waiting for the competition, at the request of some engineers, who wished to have it made known without loss of time. The method recommended consists in employing a floating air chest in the manner of a diving-bell, forcing out the water from its lower part by means of bellows, after shutting up the workmen in the upper part. In the additional paper printed in the *Journal de Physique,* a stronger pneumatic apparatus is described, somewhat resembling the air-vessel of a fire engine, which discharges its air into the chest, and is then filled again by letting out the water: it is also proposed to employ mercury, in a similar manner, in the construction of an air-pump. The proposed apparatus does not appear to have been tried: but there can be no doubt that it might often be of advantage, when the inequalities of the rock, or any other causes, prevented the construction of a cofferdam.

4. *Theory of Simple Machines, comprehending the effects of friction and of the stiffness of ropes. Mém. Sav. Etr.* X. P. 161. This essay gained the double prize proposed by the Academy for 1781; the difficulty of performing experiments on a large scale having probably prevented the presentation of any memoir sufficiently comprehensive the preceding year, when the subject was first proposed. The author's principal merit consists in the determination of the different magnitude of the initial adhesion, according to the time that the substances had continued in contact with each other and had been pressed together, and of the effect of the magnitude of the surface of contact of particular substances, as well as of the causes of the occasional difference of friction, with different velocities, especially when the unctuous substances employed are rendered too soft by the heat, which is produced by the motion. He compared the effect of the mutual contact of a great variety of substances; and for the purpose of launching ships he recommends the use of oak, sliding on elm, previously well rubbed with hard tallow; but in some other cases he found tallow, if not frequently renewed, rather injurious than service-

able. He observed, that the rigidity of ropes increased more rapidly than their diameter, but somewhat less rapidly than their strength; and that in order to overcome this rigidity, besides a constant force, an additional force was required, proportional to the weight employed. With every allowance for resistance of all kinds, he calculates that a well constructed machine, for instance a simple capstan, raising a large weight, will produce an effect equivalent to nine-tenths of the force employed. But in many of the simple machines in common use, for instance in ships' blocks of the ordinary construction, it appears, from the reports of other authors, that the loss frequently amounts to more than half of the power.

5. *Observations on the force of Windmills, and on the form of their sails. Mém. Ac. Par.* 1781. P. 65. The inclination of the sails, found to succeed best in practice, varied from 60° to 80° in their different parts. The force was estimated by the weight of the stampers, raised in the process of extracting rape oil, and it appeared that only about one-seventh of the power of the wind was lost. On an average the wind was observed to blow eight hours a-day, with a velocity of fifteen English miles an hour; and the work of the mill was generally so arranged, that the velocity of the sails was in a certain proportion to that of the wind, experience having shown, that the effect thus obtained was the greatest possible. Mr. Coulomb attempted to become a tenant of one of the mills for a few months, in order to make experiments on it with greater convenience: but the proprietors suspected that he wished to discover some of their secrets, and refused to comply with his proposal.

6. *Theoretical and experimental researches on the force of Torsion. Mém. Ac. Par.* 1784. P. 229. The force of torsion is here very accurately and elegantly determined for substances of different diameters; and it may be inferred from Mr. Coulomb's experiments, that the resistance of a steel wire, to a force tending to twist it, is always equal to that of a fixed axis, supposed to be $\frac{13}{100}$ as great in diameter, having the same wire coiled round it, or rather simply attached to a point of its circumference: for brass wire the proportion must be $\frac{8}{100}$; and according to Mr. Cavendish's experiments, it must be $\frac{1}{10}$ for

copper; not $\frac{1}{100}$, as has been stated by an accidental error either of the printer or of the calculator, in the article Carpentry (Supra, p. 251.) The reaction of brass, notwithstanding its greater flexibility, is more perfect and durable than that of steel, and it is therefore preferred for the construction of balances for the measurement of minute forces by the effect of torsion, several varieties of which are here described: And as an instance of their utility, the author has ascertained, that the resistance of liquids depends almost entirely on two forces, the one varying as the velocity, the other as its square, the constant portion of the resistance being scarcely perceptible in any case. He proves, that tempering a bar or wire, of any metal, has no effect in the immediate force of its resistance at a given flexure, although it very materially modifies the extent of its action. Continued twisting of a soft wire seems to produce a very equable degree of hardness, which enables it to retain nine times as much magnetic power as in its original state: and on account of this increase of hardness only, a soft wire appears to exhibit a greater extent of elastic recoil when it is twisted round several times, than when only once or twice.

7. *Description of a Compass.* *Mém. Ac. Par.* 1785. P. 560. The needle is suspended by a number of single threads of silk, made to adhere by dipping them in hot water, or by means of a little gum, each thread being capable of bearing a weight of about 50 grains. The needle is to be so suspended, that the thread may have no tendency to cause it to deviate from the magnetic meridian; and this is to be ascertained by substituting a copper wire in its place. Cassini was in the habit of employing a compass of this construction, for making accurate observations on the diurnal variation.

8. *Three Memoirs on Electricity and Magnetism.* *Mém. Ac. Par.* 1785. P. 569, 578, 612. The first memoir is devoted to the description of an electrical balance, founded on the force of torsion, and to the demonstration of the law, according to which small bodies similarly electrified repel each other, with a force decreasing as the squares of the distances increase. One of the instruments employed was so delicate, that each degree

of the circle of torsion expressed a force of only one hundred thousandth of an English grain; another, suspended by a single fibre of silk four inches long, made a complete revolution with a force of one seventy thousandth of a grain, and turned to the extent of a right angle, when a stick of sealing wax, which had been rubbed, was presented to it at the distance of a yard. The second memoir relates to the laws of electric attraction, and of the magnetic forces, which are all found to vary in the same proportion as the electric repulsion. The direct experiments, on the attraction of balls contrarily electrified, presented some difficulties, and the vibration of a small needle, at different distances from an electrified body, was employed for a collateral experiment. The poles, in which the magnetic forces appear to be concentrated, are at some little distance from the respective ends of a magnetic bar, and not exactly at the extremities. In the third memoir, Mr. Coulomb investigates the laws of the gradual loss of the electricity of an insulated body, which seems to be always proportional to the intensity of the charge, and independent both of the form and of the conducting power, except in the case of sharp points or edges; it appears also to vary nearly as the cube of the quantity of water contained in the air, though probably somewhat diminished by an increase of temperature. It is however remarkable, that changes of moisture, indicated by the hygrometer, are not discoverable in the conducting power, for a considerable time afterwards. The quantity of electricity carried off by the air being ascertained, that which is lost by the imperfection of the insulating support remained to be determined: and it was found, that a certain length of a fibre of silk, varying as the square of the intensity, produced complete insulation with respect to all weaker charges.

9. *Fourth Memoir on Electricity. Mém. Ac. Par.* 1786, P. 67. It is here shown that the capacity for receiving electricity is totally independent of any chemical attraction of the body for the supposed fluid; since balls of copper and of pith, or plates of iron and of paper, when brought into contact with each other, divide the electricity in equal proportions. It is also experimentally proved, by boring cylindrical holes in a

large piece of wood, and touching the bottom of the holes with a small circle of gilt paper, that the interior parts of an electrified body remain in a state of indifference.

10. *Fifth Memoir on Electricity. Mém. Ac. Par.* 1787, P. 421 When a large globe touches a smaller, the smaller receives a charge, which is stronger than that of the larger, but never twice as strong. It is proved by measuring the intensity of the electricity of a varnished wire or cylinder, that bodies are not surrounded by an electric atmosphere, but receive the charge within their substance; for the varnish, which is impermeable to the fluid, does not sensibly affect the capacity of the cylinder.

11. *Sixth Memoir on Electricity. Mém. Ac. Par.* 1788, P. 617. This interesting investigation relates to the distribution of electricity between a number of equal globes; in the different parts of a long cylinder; between a large globe and a number of small ones; and between a globe and a cylinder. In showing the agreement of the theory with experiments, Mr. Coulomb's industry and ingenuity are very successfully exerted in order to overcome the difficulties of the approximatory calculation, although it might perhaps have been not much more laborious, and yet far more satisfactory, to have proceeded to a more correct and conclusive analysis. In a series of twenty-four globes, the charge of the first was to that of the second as three to two, and to that of the twelfth as seven to four; in a cylinder fifteen diameters long, the intensity at the end was to the intensity at the middle as twenty-three to ten. Of twenty-four small globes in contact with a larger, the last exhibited an intensity about four times as great as the first. The experiments on globes and cylinders combined are still more interesting, as affording an immediate application to the effects of conductors in carrying off electricity. At the remote end of a long cylinder equal in diameter to one-twelfth of that of the globe, the intensity was nearly twenty times as great as that of the globe; and it increased almost in the same proportion as the diameter of the cylinder or wire was diminished; but a short wire received a much weaker charge. From the formulæ founded on these experiments, Mr. Coulomb calcu-

lates, that a cloud, a thousand feet in diameter, will cause a wire a line in diameter, raised by a kite, to receive an electricity at the lower end more than sixty thousand times as great as its own. He also observes, that a point projecting but little from a large surface discharges electricity but slowly; a plane touching a globe received an electricity, equally intense with that of the globe, on both its surfaces. Mr. Coulomb considers the hypothesis of the existence of two electric fluids as less objectionable than the theory of Franklin, Æpinus, and Cavendish, though he does not attempt to give any direct proof that he can decide the question; but he finds it difficult to believe, that matter can repel matter, and attract the electric fluid, with forces precisely equal, at the same time that matter is known to attract matter with a force of gravitation, varying according to the same law, but incomparably less active. It does not, however, appear that this difficulty is by any means a very important one, since we may avoid it altogether, by supposing that matter only repels matter, and that it attracts the electric fluid, with which matter is commonly saturated, with a force somewhat greater, so that the difference of these forces constitutes gravitation; which thus, like the newly discovered chemical attractions depending on electricity, may be reduced to a modification of the power of this wonderfully universal agent; an agent which appears almost to combine the subtility of spirit with the energetic qualities of matter. It must, however, be remembered, that we have no evidence of the separate existence of electricity, independently of matter; it does not pass, like light and heat, through the vacuum of the barometer; nor, in all probability, through the empty spaces interposed between the different parts of the solar system; although the accelerative force, depending on it, is not confined by these or by any other limits; and it will probably long remain a question, whether electricity may not rather be a modification of matter or motion in the bodies concerned, than a semimaterial substance pervading them; especially among those who even doubt of the materiality of light and heat as separate substances.

12. *Seventh Memoir, relating to Magnetism. Mem. Ac. Par.*

1789, P. 455 In order to check the irregular oscillations of needles very delicately suspended, Mr. Coulomb finds it convenient to attach to them a horizontal plate, immersed in a vessel of water. The directive power of a needle of given thickness appears to be nearly proportional to its length, the quantity of magnetism accumulated near the ends being constant, except that it extends to the distance of about twenty-five diameters, and if the needle is too short to allow space for this accumulation, the directive power decreases as the square of the length. The directive forces of similar needles, composed of pieces of the same twisted wire, are nearly as their weights. Mr. Coulomb observes the difficulty of accounting for the well known fact, that neither half of a needle, when it has been divided, appears to be attracted either northwards or southwards, and he thinks that whether we admit the existence of one magnetic fluid or of two, it will still be necessary to consider every magnet as made up of minute parts, each possessing a north and a south pole of intensities varying according to their situation; and he remarks that the high charges of electricity, supported by very thin plates, afford an analogy favourable to the existence of this kind of partial charge of magnetism; he might also have added, after the happy combinations of Volta, that the electrochemical battery exhibits an arrangement almost identical with that which he attributes to a magnet. With respect to the forms of needles, the rhomboid appeared to have some advantage over the rectangle: the temper required to be neither very hard nor very soft, and it was found best to anneal the needles to a dark red, or to employ plates of a spring temper, when they required to be larger. A number of needles combined into a mass lost more than half their strength, so that it is of advantage to attach several parallel needles, at a distance from each other, to the card of a compass. Mr. Coulomb's mode of communicating magnetism is to lay the ends of the bar on those of two strong magnets, placed opposite to each other, and to draw two other magnets repeatedly along it, in an inclined position, from the middle to the respective ends, in opposite directions at the same time. His large battery consisted of a number of plates surrounding two pairs of pieces of

soft iron, which formed the ends of each compound magnet, while the middle was left hollow: the whole weighed 30 or 40 pounds, and would lift 80 or 100, and it communicated to common needles as much magnetism as they were capable of retaining, when their ends were merely placed on it, without any farther operation.

13. *Examination of the friction of Pivots. Mém. Ac. Par.* 1790, P. 448. Mr. Coulomb tacitly acknowledges, in this paper, a slight inaccuracy in his former experiments on the friction of pivots, the result of which seemed to indicate that the rotatory resistance of the friction was simply as the pressure, independently of any change of the magnitude of the minute surface of contact: he now finds that this is only true of smaller weights, when the pivot has already supported a larger, and its surface has probably been a little flattened: otherwise the observation agrees more nearly with the theory; and perfectly so on the supposition of a conical point affording a resistance proportional to the displacement of the surface. The friction of steel on garnet is a little less than half as much as on steel; on agate, a little more than half; and on glass, four-fifths. Light needles, with a pivot tapering in an angle of about 20°, seem to be the most advantageous for common purposes; but if the needle is heavy, the point must be more obtuse. The conoidal caps, commonly used for suspending needles, had always an irregularity at the centre, which made the friction many times greater than that of a well finished surface, uniformly concave.

14. *Experiments on the Circulation of Sap. Mém. Inst. Sc.* II. P. 246. Mr. Coulomb seems to have sufficiently ascertained that the sap appears to rise in the poplar, near the centre of the tree, mixed with a considerably larger portion of air, which is extricated with a hissing noise, when the tree is cut or bored; it is not, however, certain that this air is in an elastic state while the vessels remain closed. The phenomenon was first observed in April, and continued throughout the summer, being most distinct in the hottest sunshine.

15. *Observations on the Daily Labour of Men. Mém. Inst. Sc.* II. P. 380. This memoir was read to the Academy of

Sciences in 1775, but was not then published. Mr. Coulomb's general conclusion is in favour of the employment of strength in ascending stairs; but he observes, that former authors have very frequently exaggerated the whole amount of a man's daily labour. In fact, the day's work, which he assigns to a man of ordinary strength, thus employed, is less than half of that which Desaguliers attributes to a labourer turning a winch: and Professor Robison has recorded, in this *Encyclopædia*, more than one instance of a much larger result of the labour of a man ascending an inclined plane, even besides the force lost in the machinery employed: so that we must suppose the labourers in France to be commonly less vigorous than in Great Britain; almost in the same proportion, as Mr. Coulomb has observed the work of the same man in Martinique to be less than in France.

16. *Comparison of the Magnetic Powers of different Needles. Mém. Inst. Sc.* III. P. 176. A number of accurate experiments are here adduced, in confirmation of the theoretical conclusion, that needles of a similar form, and composed of portions of the same wire, possess directive powers which are very nearly proportional to their weights.

17. *On the Cohesion of Fluids and their Resistance to Slow Motions. Mém. Inst. Sc.* III. p. 246. The interesting expements here related, demonstrate that the constant part of the resistance of fluids is insensible: that the portion varying simply as the velocity is more than seventeen times as great in oil as in water, while the portion which varies as the square of the velocity, is nearly equal in both these fluids. The resistance did not increase with the depth of immersion; on the contrary, it was a little greater when the body was partly above the surface. It was observed that very slow oscillations were somewhat accelerated even by the motion of a carriage passing along the street. Greasing the surface of the solid did not sensibly lessen the resistance to its motion, nor was it materially increased by sprinkling sand on the grease; so that the particles of the liquid seem to slide rather on each other than on the solid. But it is probable that these differences would have been more perceptible in greater velocities; for it seems reason-

able to expect, that the friction between a fluid and a solid should partake, in a slight degree, of the nature of the friction between two solids, so as to increase less rapidly, with an increase of velocity, than the friction of the particles of fluids among themselves.

18. *A new method of determining the Position of the Dipping Needle. Mém. Inst. Sc.* IV. P. 565. The method, suggested in a former memoir, is compared, in this short essay, with the mean of four observations made in the common way, the magnetism of the needle being reversed during the experiment; and it appears that the error of either method is not likely to exceed ten or twelve minutes.

19. *On Universal Magnetism. Bullet. Soc. Philom* N. 61. 63. *Journ. Phys.* LIV. P. 240. 267. 454. *Journ. R. Inst.* I. P. 134. The experiments mentioned in the first of these papers were immediately repeated in this country, with results less satisfactory than those which Mr. Coulomb had obtained; and he soon found reason, upon a further examination, to change the opinion which he had at first inferred from them, observing that a grain of iron was sufficient to communicate sensible magnetism to twenty pounds weight of another substance. There still remain, however, some difficulties respecting the magnetism of brass which have not yet been sufficiently explained, and which have been mentioned in the article CAVALLO of this *Supplement.*

Mr. Coulomb's moral character is said to have been as correct as his mathematical investigations. At an early period of his life he gained the grateful acknowledgments of the inhabitants of Britanny, for his disinterested exertions, in preventing the execution of some public works which threatened to be ruinous to the province. His manners were serious, but gentle, and sometimes diversified by a mild gaiety, which made him very amiable in society. His disposition was generous and benevolent; but notwithstanding all his modesty, he could exhibit sufficient spirit, when he was called upon to repel an unjust attack. Such occurrences were, however, far from being frequent, for his merits and his success were universally acknowledged, and he was extremely popular, without ever

having excited envy. In the particular department of science which he cultivated, he may fairly be ranked in the same class with Franklin, Æpinus, and Cavendish. He was less original than Franklin, but much more profound. He gave to the speculations of Æpinus both a more defined application, and a more satisfactory demonstration; and he was equally accurate with Cavendish, but much more persevering with respect to the more limited objects of his researches; and his improvements in the theory of electricity may be considered as having immediately prepared the way for the elegant inventions of Volta, and the still more marvellous discoveries of Davy. In short, among all the men of science who have done honour to France, it would be difficult to point out a single individual who, with regard to the cultivation of terrestrial physics, could at all be put in competition with Mr. Coulomb. His health had long been extremely feeble, and in addition to his more chronic complaints, he was at last attacked by a slow fever, to which he fell a victim on the 23rd of August, 1806. He had been a Lieutenant-Colonel of Engineers, a Chevalier of the Order of St. Louis, and a Member of the Legion of Honour; but he had acquired little property; and he left to his two sons scarcely any other patrimony than the public gratitude and esteem, for his merits and his virtues.

No. LXXVII.

LIFE OF BORDA.

JOHN CHARLES BORDA, a Mathematician and Nautical Astronomer, celebrated for his improvements in the theory of Hydraulics and Pneumatics, and in the construction of instruments for observation. He was born at Drax, the 4th of May, 1733, and was originally destined for the bar, but abandoned the pursuit of the law in favour of a military life, which he considered as better calculated to afford him opportunities for the cultivation of his mathematical talents, and for the application of the results of his studies to practice. His acquirements in science had very early attracted the attention of D'Alembert, who predicted his future eminence, and warmly recommended his turning his thoughts to the occupation of a place in the Academy. He obtained a commission in the Light Cavalry, and was appointed Teacher of Mathematics to the corps; and, in 1756, he presented to the Academy of Sciences (A.) *A Memoir on the Paths of Bombs*, which was ordered to be printed in the collection of the *Savans Etrangers*, but which has not excited much attention. He was elected in the same year a member of the Academy; and in the next he was present at the battle of Hastinbeck, in the capacity of Aide-de-Camp to the celebrated General Maillebois, to whom he looked up as a great master in the art of War.

He was afterwards admitted into the corps of Engineers, without the usual form of examination into his qualifications; and, being stationed at a seaport, the occurrences of the place naturally directed his attention anew to the phenomena of the resistance of fluids. He published, in 1763, a detailed memoir on this subject (B. *Mém. Ac. Par.* 1763, p. 358), in which he relates a variety of experiments, showing, that the resistance of the air is actually proportional to the square of

the velocity, as had commonly been supposed from theoretical considerations. He also determines, by other experiments, the magnitude of the resistance to the motion of a sphere, and proves, that nothing can be more erroneous than the supposition, that the resistance to an oblique surface decreases as the square of the sine of the angle of incidence. He also finds, that the resistance to the motion of a cube, in the directions of the diagonal of its base and of one of the sides, are as 21 to 16, while the calculations of former theorists had made the resistance greatest in the direction of the side.

In 1766, he published an Essay on the discharge of fluids through the orifices of vessels (C. *Mém. Ac. Par.* 1766, p. 579), in which he first states the objections to considering the different strata of a fluid as descending in all cases very nearly in parallel directions; he examines the contraction of the jet after its escape from the orifice, and determines some of the effects of abrupt changes in the velocity of the fluid passing through pipes or apertures of different forms.

He contributed, in 1767, to the publications of the Academy, an important *Memoir on Water Wheels* (D. p. 270), which has escaped the notice of his able Biographer M. Lacroix. He observes, in this paper, that the simple hypothesis of a resistance varying as the square of the velocity, which is so near the truth in common cases, where a number of particles, proportional to the velocity, strikes, in a given time, upon a small exposed surface with a force also proportional to the velocity, is totally inapplicable to the action of a confined stream upon the floatboards of a wheel, since, in this instance, the number of particles concerned cannot vary materially with the velocity, the whole stream being supposed to operate in all cases upon the successive floatboards; so that the analogy would require us to suppose the force in this case nearly proportional to the simple relative velocity; a conclusion which agrees remarkably well with the experiments of some practical authors.

The same volume contains a continuation of M. Borda's researches relating to the resistance of oblique surfaces (E. *Mém. Ac. Par.* 1767, p. 495), with a statement of experiments still more conclusively confuting the received hypothesis,

respecting oblique impulse, than his former investigations had done. We also find in it an Essay on isoperimetrical problems (F. p. 551), in which it is shown, that Euler's method of treating them, which had been in great measure abandoned by its equally profound and candid author, in favour of the more general and more elegant calculations of Lagrange, was still capable of affording all the results that had been derived from the method of variations; and he even pointed out some deficiencies in the first Memoir of Lagrange, which contained the detail of his ingenious invention. These investigations of M. Borda afford collateral evidence of the strict truth of the demonstrations of both his great predecessors; and though they have been little employed by later Mathematicians, yet it must be admitted to be of some importance, in enabling us to appreciate the value of a new mode of calculation, to determine whether its results are or are not such, as might be obtained, with almost equal convenience, by methods before in use.

His memoir, inserted in the collection of the Academy for 1768 (G. *Mém. Ac. Par.* 1738, p. 418), is devoted exclusively to the theory of pumps; and he considers especially the effect of the passage of the fluid through valves and other contracted parts, in diminishing the quantity of the discharge. His results are derived from the principle of the preservation of the living force or energy of a system of bodies, throughout all the vicissitudes of its motions, which had before been employed with success by Daniel Bernoulli in problems of a similar nature; but it was not until the experiments of Buat had afforded sufficient grounds for the determination of the friction of fluids, that cases of this kind could be submitted to exact calculation.

In his Essay on the curve described by cannon-balls, published among the Memoirs for 1769 (H. *Mém. Ac. Par.* 1769, p. 247), he has greatly simplified the practical theory of projectiles, which had been treated in a satisfactory, though very general manner by John Bernoulli, and had been reduced into a much more convenient form by Euler. M. Borda has substituted some approximate expressions for the true value of the density of the air, and has thus been enabled to integrate

equations which, in their more strictly correct form, had resisted the powers of Euler himself; and he has justified the adoption of the formulas thus obtained by a comparison with experiment.

In the mean time his talents were very actively employed in the naval service of his country, which he entered in 1767, by the nomination of M. Praslin. The time-keepers of Leroy and Berthoud were beginning to rival those of the English artists, and the French Government ordered several vessels to be fitted out for cruises, in order to examine the accuracy of these time-keepers. M. Borda was appointed a Lieutenant on board of the *Flore*, and acted jointly with M. Pingré as a delegate of the Academy of Sciences for the purposes of the expedition. The voyage occupied about a year, and extended to the Canaries, the West Indies, Newfoundland, Iceland, and Denmark. M. Borda had a considerable share in the account which was published of the observations; and the formula, which he has here given, for the correction of the effects of refraction and parallax, is considered as equally elegant and convenient. He also presented to the Academy a separate Memoir on the results of the expedition. (I. *Voyage pour éprouver les montres de Leroy*. 4. *Paris*.) (K. *Mém. Ac. Par.* 1773, p. 258.) After an interval of six weeks, these watches were found capable of determining the longitude within about fifteen minutes of the truth.

In order to supply some deficiencies in the observations made at the Canaries, Borda was sent out a second time, with the *Boussole* and the *Espiègle*, and he published, after his return, (L.) a very correct and highly finished map of these islands. He was soon afterwards promoted to the rank of Captain, and served under the Count d'Estaing as a Major-General, an appointment nearly similar to that of our Captains of the fleet. In this capacity, he observed the inconvenience of too great a variety in the sizes of the vessels constituting a fleet, and proposed to abolish the class of 50 and of 64 gun ships, as too small for the line of battle, and to build ships of three rates only, the lowest carrying 74 guns, so that a smaller quantity of stores should require to be kept ready for use in the dockyards,

than when ships of more various dimensions were to be refitted. In 1780, he had the command of the *Guerrier*, and in 1781 of the *Solitaire*, which was taken, after a gallant resistance, by an English squadron. He was thus compelled to pay a visit to Great Britain, but was immediately set at liberty upon his parole.

He proposed to the Academy in this year (M. *Mém. Ac. Par.* 1781), a mode of regulating elections, which was adopted by that body. Its peculiarity consisted in having the names of the candidates arranged by each voter in a certain order, and collecting the numbers expressing the degrees of preference into separate results, so that the simple majority of voters did not necessarily establish the claim of any individual, if he was placed very low in the list by any considerable number of those who voted against him. But, it must be allowed, that this mode of election is by no means wholly unobjectionable.

M. Borda appears to have rendered an essential service to the cultivators of Practical Astronomy, by the introduction and improvement of the repeating circle, although this instrument has probably been less employed in Great Britain than elsewhere, on account of the greater perfection of those which were previously in common use. It had been suggested by Mayer, in 1767, that a circle with two moveable sights, would enable us to observe a given angle a great number of times in succession, and to add together the results, without any error in reading them off, and thus to obtain a degree of precision equal to that of much larger and better instruments of a different construction; but the proposal had been little noticed until ten years afterwards, when Borda pursued the path pointed out by Mayer, and trained Lenoir, then a young and unlicensed artist, to the execution of the improved instrument, notwithstanding the opposition of the rival opticians, and the want of encouragement from the opulent public. He published, in 1787 (N.) his *Description and Use of the Reflecting Circle, with different Methods for Calculating the principal Observations of Nautical Astronomy;* but the officers of the French navy, for whom this work was intended, appear to have profited but little by his instructions. His instrument was, however, much employed in

the operations for determining the length of the terrestrial meridian, and he himself took charge of the experiments required for ascertaining the length of the pendulum, and for the comparison of the different standards with each other. He invented some very ingenious methods of overcoming the difficulties which present themselves in the pursuit of these objects; but he was interrupted in his researches by the horrors of the Revolution, nor did he live to see the whole of the operations completed. He endeavoured, also, to promote the introduction of the new mode of subdividing the circle, by the laborious computation of *Tables of Logarithms* (O. 4to. *Par.* 1801), adapted to decimal parts of the quadrant,—a work in which he was assisted by M. Delambre. From the increasing indisposition of M. Callet, who had undertaken to correct the proofs of these tables, some very material errors had been committed in the first half of the tables, and M. Borda thought it necessary to cancel a great number of the pages; and in order to meet the expense thus entailed on him, he was obliged to dispose of an estate which he had lately acquired in his native place. He was also engaged, towards the close of his life, in the measurement of the force of magnetism, and in the calculation of astronomical refraction. His health had been threatened for several successive winters, and he died the 10th of March, 1799.

In his manners he was animated and unaffected: he avoided those who sought his acquaintance merely from the vanity of being intimate with a man of talents, whatever pretensions to importance they might derive from their casual relations to general society. He never married; and he was too much absorbed in the pursuit of science to associate with a very extensive circle even of private friends. Though not a man of learning, he was not deficient in literary taste, and he was, in particular, a passionate admirer of Homer. He seems to have possessed a considerable share of that natural tact and sagacity, which was so remarkable in Newton, and which we also discover in the works of Daniel Bernoulli; enabling them, like a sort of instinct, to elude the insurmountable difficulties with which direct investigations are often encumbered; while Euler, on the

contrary, as M. Lacroix most truly observes, seems to have taken pleasure in searching for matter which would give scope to his analytical ingenuity, although wholly foreign to the physical investigations which had first led him to the difficulties in question. It would have been fortunate for the progress of science, if some of the most celebrated of M. Borda's countrymen had profited by his example, in studying to attain that unostentatious simplicity which is the last result of the highest cultivation.

No. LXXVIII.

LIFE OF DE LA CONDAMINE.

Charles Maria de la Condamine, a practical geographer and cultivator of science in general; son of Charles de la Condamine, a Receiver-General of Finances, and Margaret Louisa de Chources; was born the 28th of January, 1701.

His early education was by no means neglected; although he complains, in a manuscript memoir which he left respecting the progress of his studies, and the developement of his faculties, that he was made to learn, as boys frequently are, too much by rote, without understanding the complete sense and bearing of the words which he repeated. It is, however, by no means certain, that any great loss of time is ultimately incurred by this practice; for in fact the memory is much strengthened by the constant habit of getting by heart: and it does not appear that the judgment is at all impaired by it. He afterwards pursued his studies under Father Brisson, and in 1717 supported a thesis on the Cartesian philosophy, which the Jesuits were then beginning to introduce into their seminaries, while it was elsewhere giving way to the Newtonian. In 1719, after he had left college, he entered the army, and accompanied his uncle, the Chevalier de Chources, to the siege of Rosas, as a volunteer; and both on this and other occasions, he exhibited sufficient proofs of the contempt of danger, and the spirit of enterprise, which were so much required in those pursuits, which afterwards occupied a considerable portion of his life. Notwithstanding the dissipations in which military men are very commonly involved, he was moderate in the pursuit of pleasure; and he used to consider the distortion which the small-pox had left in his features, as having afforded him some compensation for the injury done to his vanity, by diminishing the temptations to which his sensibility might otherwise have exposed him.

Having no prospect of speedy advancement in the army, and having suffered considerably in his fortune from a participation in the extravagant speculations of Law, he quitted the service, in hopes of finding a more advantageous employment in science. He distinguished himself as an active member of a society of arts, then recently established at Paris by the Count de Clermont; and in 1730, he obtained a situation in the Academy of Sciences, as Adjunct of the class of Chemistry, having previously presented to the Academy a memoir on the mathematical and mechanical properties of the lathe, which obtained him considerable credit. Soon afterwards he embarked in the squadron of Duguay Trouin, and made a voyage in different parts of the Mediterranean; he passed several months at Constantinople, and visited the plain of Troy and many other parts of the Levant; after his return he gave an account of his tour to the Academy; and a servant who had accompanied him published also a separate journal of his own.

Chemistry, as it was cultivated at that period, afforded but little scope for the employment of an active mind, and La Condamine, after the publication of one chemical memoir only, was removed from the class of chemists in the Academy to that of astronomers. In this capacity, he was the first to propose the measurement of a degree of latitude in the neighbourhood of the equator: and he seems to have felt the importance of the undertaking, not only for the purpose of deciding a great question in science, but also for that of attracting the attention of the public, and establishing the system of gravitation in France by a grand operation, executed with great labour and at a great distance, by Frenchmen only. His ideas were readily seconded by Maurepas, then prime minister; and he was appointed by the Academy, together with Bouguer and Godin, for carrying the proposal into effect.

In this expedition he was absent from his country for nearly ten years, from 1735 to 1745; and he had to combat with difficulties of every kind: a distant voyage; an uncivilized and sometimes uninhabited country; impracticable roads; want of regular remittances; the necessity of disposing of valuable articles disadvantageously, in order to procure a temporary sup-

ply; a malicious prosecution upon the pretence of a contraband traffic, founded only on this circumstance; and the still more violent attacks of a fanatical mob, who murdered the surgeon of the expedition; all these things, to say nothing of the awful appearance of an eruption of Cotopaxi, and the no less formidable operations of the hostile squadron of Lord Anson, required nothing less than the dauntless spirit and energy of character which he possessed to bear him up against them; and at last the little jealousies, which will often arise among persons of science, employed in the same pursuits, embarrassed and embittered the conclusion of his enterprise. The activity and fluency of La Condamine made the public disposed to imagine, that Bouguer had been only his humble attendant; and Bouguer was too conscious of his own superiority as a mathematician, to bear this injustice with patience: he complained, but the laugh was against him; and he revenged himself by refusing all communication in the statement of the results of the operations: so that each observer gave ultimately a separate account of his own measurements and calculations.

In consequence of all the fatigues and vicissitudes to which La Condamine had been exposed, he became extremely deaf, and partially paralytic after his return; but the powers of his mind appear to have remained unimpaired.

In 1748 he was elected a Foreign Member of the Royal Society of London; and he afterwards exerted himself with great zeal and success in promoting, among his countrymen, the general introduction of the variolous inoculation, which had long been practised in England, and in some other parts of Europe. In 1757 he took a journey to Italy, and spent a considerable time at Rome, partly with a view to the improvement of his health, and to the observation of a variety of facts connected with his scientific pursuits; but principally, perhaps, in order to obtain a dispensation from the Pope for a marriage with his niece, who seems to have had a high respect for his talents, and even a sincere attachment to his person, notwithstanding the disparity of their ages, and the caprices of a temper not a little impatient and irritable. He became, in 1760, one of the forty members of the French Academy, and contri-

buted considerably to an improved edition of their Dictionary. In 1763 he paid a short visit to England, which was rendered less agreeable to him on account of the difficulty that he found in obtaining legal redress for some slight injury which he had received: after his return, the insensibility of his limbs increased, and he was obliged to relinquish all his pursuits of science, retaining only the amusement of making some light attempts in poetry, and occasionally inserting in the periodical works of the day a few tales in verse, besides a poetical translation of a part of Virgil's *Eneid;* although it seems natural to suppose that the exercise of the inventive fancy of a poet would tend to exhaust the debilitated faculties, even still more, than the methodical investigation of mathematical or physical subjects.

His publications embrace a very great variety of subjects: the following appear to be the most deserving of notice:*—

1. *On the Determination of small Differences of Longitude*, M. Ac. Par. 1735, p. 1. A discussion of the kinds of signals best adapted for contemporaneous observations at a distance; a subject much more difficult at that time than at present, when the art of pyrotechny has been carried to so high a degree of perfection, especially since the late singular invention of a rocket with a parachute, descending slowly and exhibiting successive explosions.

2. *Measurement of the Length of the Pendulum at St. Domingo*, M. Ac. Par. 1735, p. 529. The academicians were detained at St. Domingo more than three months, and they took advantage of the delay in order to ascertain the length of the pendulum in that latitude. Mr. de la Condamine employed a ball of brass, suspended by a thread of the aloe, and attached to it by means of a piece of sticking plaster; the pendulum making a vibration in about two seconds, he observed in how many vibrations a second was lost or gained. The length thus determined, was 36 French inches 7¼ lines, or 39.0125 English inches; Messrs. Godin and Bouguer made it one-twelfth of a line longer, or 39.020, and a calculation from

* The more detailed notice of his publications which is given in the original has been abridged.—*Note by the Editor.*

the best modern observations, for the latitude, which was 18° 27′, gives us 39.029. The thread was fixed by a clip, and it was probably very flexible, since its rigidity must necessarily have tended to increase the curvature of the path of the ball, and to give a measure somewhat too long. See the article COHESION. (Vol. I., No. XX.)

3. *Abstract of a Journey through a part of South America*, M. Ac. Par. 1745, p. 391. H. 63. After the completion of the operations at Quito, Mr. de la Condamine determined to take the course of the river Marañon for his return towards Cayenne; and in the course of this route, of more than 2000 miles, which he performed partly by land, but principally on a raft, he had an opportunity of making a multitude of interesting observations of various kinds. He found in several places a singular agreement of traditions respecting the former existence of a Republic of women only, in the neighbourhood of the river which has received its most usual denomination from them. He observed the effect of the tides at Pauxis, a point 600 miles from the mouth of the river, but not much elevated above it; and he was informed that there were always a number of alternations of high and low water at the same time, between this point and the mouth of the river; in some places where the water was shallow, he encountered the tide rising in the form of a bore, called by the French a *barre* or *mascaret*, and by the inhabitants there a *pororoca*, occupying but one or two minutes in its ascent, and frequently producing accidents to boats unprepared for its reception. After his arrival at Pará, he measured the length of the pendulum there, and found that the force of gravitation was about $\frac{1}{1000}$ greater than at the summit of Pichincha. He next proceeded to Cayenne, and was obliged, on account of the war, to return to Europe by a Dutch ship which sailed from Surinam.

4. *Abstract of the Geographical Operations performed in South America*, M. Ac. Par. 1746, p. 618. The length of a degree at the equator appears, from these calculations, to be 56,750 toises; Bouguer, who employs the same determination of the arc, but a different series of trigonometrical observations, makes it 56,753; and Godin, on the other hand, some-

what less than La Condamine. This is more than 300 toises less than the degree measured in France, and almost 700 less than the degree in Lapland; and it gives for the earth's ellipticity, by comparison with the former, $\frac{1}{303}$, and with the latter $\frac{1}{213}$. The terminations of the base were marked by pyramids, and the length of the toise was identified by a bar of metal, let into a tablet of marble, with an appropriate inscription.

5. *A History of the Variolous Inoculation.* M. Ac. Par. 1754, p. 615. A candid, clear, and judicious statement of the advantages of the inoculated above the natural small-pox, in a popular and sometimes even playful style, calculated to meet the prejudices of the day and the various superstitious and interested motives which retarded the practice in France, while it had become universally prevalent in England; although, in more recent times, the public spirit in this country appears to be somewhat less favourably disposed to the admission of beneficial innovations; for scarcely in any part of the world has vaccination become less universal than it is at this time in Great Britain.

6. *Abstract of a Journey in Italy*, M. Ac. Par. 1757, p. 336. H. p. 6. From an examination of several ancient standards, and from a comparison of the remains of buildings supposed to have occupied a certain round number of feet, Mr. de la Condamine concludes that the old Roman foot was equal to 130.9 French lines; that is, to 969 thousandths of an English foot. Mr. Folkes had before made it 966; but Mr. Raper has shown, in the *Philosophical Transactions* for 1760, by a very careful comparison of a multitude of documents, that before the reign of Titus, it somewhat exceeded 970, and under Severus and Diocletian, it was less than 965; the original standard in the temple of Juno Moneta having probably been destroyed by fire. Mr. de la Condamine also viewed the races on the Corso with an eye equally mathematical, and observed that the Barbary horses ran at the rate of about 40 English feet in a second; but his correspondents in England furnished him with unexceptionable evidence, that the horse *Childers* ran the four mile course at Newmarket at the rate of very nearly 50 English feet in a second, while no other horse exceeded 48;

and he observes that, in this instance, truth far outruns probability; a remark which has been somewhat misrepresented in this country, and converted, by the lovers of the amusements of the turf, into a laugh against the lovers of the amusements of science; the story being told as if the French mathematicians had demonstrated the absolute maximum of a horse's utmost possible speed; and a bet having been made on the occasion, an English horse had been found that actually exceeded the maximum. Our author also notices the awkward effect of the Roman mode of beginning the day at sunset, which renders it necessary to make continual alterations in the clocks, directions being given in the Almanacks for putting them forwards or backwards a quarter of an hour at a time; and the precise time of noon happening in summer at 16 o'clock, and in winter not till 19. He observes that a single signal, properly placed on the Apennines, would be visible at once near Trieste and near Monaco, giving a difference of longitude of not less than five degrees.

In 1768 his name is mentioned as having excited the attention of the members of the Academy by a relation of Spallanzani's experiments on the reproduction of the heads of snails, which several of them repeated with success. In fact, there was scarcely any one of the sciences to which he did not occasionally render some service, although he wanted patience and perseverance to make any very important discoveries or improvements, by his individual exertions only. But his knowledge was universal; he understood and wrote all languages; he corresponded with men of celebrity in all countries; he published upon all subjects; he contributed to all the literary and scientific journals of the day; he answered all criticisms, and he accepted all compliments, even from persons that he despised; for he delighted in the parade of a pre-eminent reputation. His style was simple and natural; a little negligent, but still elegant and lively; his manner was animated and somewhat singular; his temper was warm and restless; he sighed for repose, and was incapable of enjoying it; thinking nothing that occurred indifferent to him, and allowing none about him to be idle. He obtained the rank of Chevalier in several

orders, and was a member of several foreign academies; he had also the appointment of honorary secretary to the Duke of Orleans. At the age of 68, he addressed to his wife an account of his education, and of the earlier progress of his mental faculties, as a practical illustration of his opinions respecting the cultivation of the mind: the memoir was not published; but it is perhaps unfortunate for mankind that men of celebrity seldom find a sufficient motive for communicating candidly to the world the results of such a self-examination. A few years before his death, he printed a memorial in behalf of Mr. Godin, who had been reduced to indigent circumstances; and he had the pleasure of obtaining for him the assistance which he required. He suffered occasionally from a hernia, and having read of the marvellous cures which some empiric professed to have performed by the application of a caustic, he determined to make the experiment on himself, without the knowledge of his family, and without much hope of success; but he fell a victim to the courage or the curiosity, that prompted him to submit to the operation. In the course of the six weeks that he survived, he was still employed in writing or dictating a memoir containing answers to some questions respecting the manners of the Americans. He died on the 4th February, 1774, leaving many of his books and instruments by will to the Academy of Sciences. (*Hist. Acad. Par.* 1774, p. 85.)

No. LXXIX.

LIFE OF LAGRANGE.

Joseph Lewis Lagrange, a mathematician and astronomer of the first rank, born at Turin, 25th January, 1736, was the son of Joseph Lewis Lagrange, treasurer at war, and Maria Theresa Gros, only daughter of a rich physician at Cambiano.

He was the eldest of eleven children, but nine of them died young. His family was of French extraction on both sides, and his French biographers have dwelt with pleasure on the minute particulars of their emigration, in order the more fully to authenticate their own claim to the honour of calling him their natural as well as adopted countryman. It was his great-grandfather that first settled at Turin, in the service of Emmanuel II., who married him to a Roman lady of the family of Conti. They had at one time acquired considerable affluence; but his father had ruined himself by his expenses and speculations; and Lagrange used frequently to observe, that he owed his own success in life to his father's misfortunes, since, if he had been rich, he should never have applied to the mathematics as a profession. The classics were at first his favourite study at the College of Turin: he began his scientific education with reading the works of the ancient geometricians, and at first preferred their methods of investigation to the more modern analysis; but being convinced, as it is said, by a paper of Halley in the *Philosophical Transactions*, of the superiority of the algebraical mode of representation, he applied with redoubled ardour, at the age of seventeen, to the study of the later improvements in the methods of investigation; and in his subsequent works, he abandoned, wherever it was practicable, all geometrical considerations, and seems to have valued himself on having produced a complete system of mechanics, free from the incumbrance of any diagram whatever.

When he was only nineteen, he was made Professor of Geometry in the Royal School of Artillery, but not before he had exhibited, in his first publication, a specimen of the improvements which he was throughout his life to contribute to the mathematical sciences.

The friendships which he formed with his pupils, most of whom were his seniors, led to the establishment of a society which afterwards received the sanction of the royal authority, and to the publication of their memoirs, in which Lagrange not only took the most active part as a contributor of original papers, but also by materially assisting in the demonstrations of Foncenex, and promoting the researches of Cigna and Saluces. Foncenex was soon rewarded by being placed at the head of the maritime establishment which the king was then forming; and Lagrange received in a short time a still more flattering remuneration, in the panegyrics, which were liberally bestowed on him by his great rivals Euler and D'Alembert; the former procured him, in 1759, the compliment of being made a foreign member of the Academy of Berlin, having become well acquainted with his merits by an epistolary intercourse, which began as early as the year 1754, when Lagrange communicated to him his first ideas of the solution of isoperimetrical problems, which Euler had the delicacy to allow him time to complete, before the publication of his own further researches on the subject.

In 1764, he obtained a prize from the Academy of Sciences at Paris for a memoir on the difficult subject of the libration of the moon, having treated it by an original method, derived from the principle of virtual velocities, which he afterwards applied so successfully to other branches of mechanics. Soon after this time he found an agreeable relief to the monotony and retirement of his life at Turin, in accompanying his friend the Marquis Caraccioli, who was appointed Ambassador at the Court of London, as far as Paris, where he had the delight of becoming personally acquainted with a number of the most distinguished mathematicians of the age, who were capable of appreciating his merits, especially with Clairaut, D'Alembert, Condorcet, Fontaine, Nollet, and the Abbé Marie; but in-

disposition prevented his going on to England, as he had intended, and he returned to Turin after a short stay in France. A second prize, on the subject of the satellites of Jupiter, was awarded him in 1766; and the same tribute was again paid to his merit on three subsequent occasions. It was in this year that he was invited to Berlin, as a successor to Euler in the place of Mathematical Director of the Academy, Euler having been induced to remove to St. Petersburg, by a better prospect of providing for his numerous family. The appointment of President of the Academy, held by Maupertuis, had been given but in part to Euler; the whole was offered to D'Alembert, who declined it; but both he and Euler united in recommending Lagrange as the fittest person for the situation. It was, however, with some difficulty that he obtained his sovereign's leave to quit Turin; and the favour was at last granted to him partly in pique, on account of the terms of the invitation, which expressed the desire of the "greatest king in Europe" to have the greatest mathematician at his court.

At Berlin he pursued his career of study in tranquillity and without interruption, upon a competent income of about 300*l.* a year, with the advantage of such demonstrations of the royal protection, as were still more important than income to his rank in society. The king seems to have preferred him to Euler, as more tolerant in his opinions, though by no means joining in all the innovations of the day, and rather avoiding every discussion relating to them, as well as any great familiarity with his patron. He was made, in 1772, one of the eight foreign associates of the Parisian Academy. He is said to have married more for the sake of complying with the universal custom of his friends and colleagues at Berlin, than for any desire of female society; and he invited a relation of his own from Turin, who became his first wife; but she was soon after carried off by a lingering disease. He was about this time very closely employed on his greatest and best work, the *Mécanique Analytique;* but it was with some difficulty that the Abbé Marie found a bookseller at Paris, who agreed to undertake its publication, and only upon condition of engaging himself to divide the loss, in case of failure in the sale. He also

procured the valuable assistance of Mr. Legendre as a corrector of the press.

Upon the death of Frederick, in 1786, Lagrange no longer felt the same interest in remaining at Berlin, though he was not treated by the new court with anything like disrespect. While the ministers of Naples, Sardinia, and Tuscany, were making him offers on behalf of their respective sovereigns, Mirabeau persuaded the French ambassador at Berlin to recommend Mr. de Vergennes to invite him to Paris; but it was in reality through Mr. de Breteuil's interest, and at the suggestion of the Abbé Marie, that he was ultimately induced to settle there, in 1787, having received a grant of an income equal to that which he had enjoyed at Berlin, under the name of a veteran pensioner of the Academy, with a vote in its deliberations. He was kindly received by Marie Antoinette, on account of his connexion with Germany; and until the Revolution, he had the use of apartments in the Louvre.

It was at this period of his life, when his success had been the most gratifying, and his fame had become perfectly established, that he appeared to suffer under a degree of melancholy or apathy, which was absolutely morbid. He confessed that all his enthusiasm was extinguished, and that he no longer felt the least relish for mathematical researches. He had not even the curiosity, for two years, to open the printed volume of his *Mechanics*, which he had never seen except in manuscript. It is a consolation to think that this annihilation of his energies was only partial and temporary: he amused himself in the mean time with metaphysics, "with the history of religions," and of languages, and with medical and botanical, and especially chemical studies; and the alarms and agitations of the Revolution, which soon followed, instead of overwhelming his broken spirits, seemed to have roused his dormant powers, and to have revived his satisfied ambition, exciting him to new labours and new triumphs.

In 1791, his name appeared on the list of the foreign members of the Royal Society of London. Mr. Maurice has asserted, that all the scientific bodies of Europe, *except* the Royal Society, received him with open arms: if the remark was

intended as a censure of that Society, it is right that its injustice should not pass unnoticed.

Notwithstanding the public embarrassments which attended the Revolution, Lagrange's pension was confirmed by the National Assembly, upon the proposition of Mr. Duséjour, in the most flattering manner: and when the depreciation of the currency materially reduced its amount, he received a partial indemnification, by being appointed a member of a committee for examining useful inventions, and afterwards a director of the Mint, in conjunction with Berthollet and Monge: but this employment he found too laborious, and resigned it six months afterwards. He was greatly interested, at this period, in the establishment of the new system of weights and measures: he was so violently bent on *decimation*, that he scarcely forgave Borda for having made a measure of a quarter of a metre, and he thought so little of the advantage of integral subdivisions, that he sometimes declared he should have preferred the number 11 to 12, for the very reason that it admitted no subdivision at all, and caused all lesser quantities to be expressed in units comparable to each other only. This opinion seems, however, to have been advanced rather as an exaggerated objection to the introduction of 12, which was suggested by some more ardent innovators, than as seriously attributing a real advantage to the employment of a prime number.

When the Academies were suppressed, the Jacobins purified the commission of weights and measures by striking out the names of many of its most distinguished members, while they retained that of Lagrange, probably because he was of no political party whatever, and had always been particularly cautious in expressing his sentiments of the events of the day. In October, 1793, however, a decree was passed which ordered all persons, not born in France, to leave the country. Guyton, who was a member of the Committee of Public Safety, advised him to claim an exemption from its operation, by a requisition of that committee, on the pretext of his being employed in preparing a report on Dr. Hutton's *Treatise on Gunnery*; and he actually received an injunction from the committee, requiring his stay, "in order to complete the calculations which he had

undertaken respecting the theory of projectiles." He was attempting to reunite the experiments of Dr. Hutton with a more correct theory than had before been applied to them; but he published nothing of importance on the subject. After the murder of Bailly and Lavoisier, he had agreed to return to Berlin, and to resume his former situation there; and he was on the point of obtaining a passport, and even a public mission from Hérault de Sechelles. But the establishment first of the *Normal School*, in which he was a professor, and then of the *Ecole Polytechnique*, induced him to remain at Paris, and again directed his activity into its ancient channels. In the Normal Schools the masters were mixed with their pupils, in order that the facilities of conversation might produce a development of the subjects discussed in the most elementary manner that was possible: but the conversation was by no means supported in the form of incessant questions and answers: Lagrange's explanations were often interrupted by moments of silence, in which his inventive faculties were deeply engaged in reflection, and the whole of his powers were concentrated on a new train of ideas. It was amidst these discussions that the *Theory of Analytical Functions* originated: a work certainly not destitute of the marks of great mathematical talent, but which, when considered as a substitute for the method of fluxions and its kindred doctrines, resembles very much the suggested introduction of an undecimal in preference to a duodecimal scale of notation, with which the author had before amused himself.*

Upon the re-establishment of the Institute, Lagrange was made one of the original members; and he was the first on the list of the Board of Longitude, which was then first instituted at Paris. He received about this time a compliment highly grateful both to his love of fame and to his filial affection, in the person of his father, then past 90, and continuing to reside at Turin. By the direction of Talleyrand, who was Minister for Foreign Affairs, the Commissary of the Directory in Piedmont, attended by the generals of the French army, and several other persons of distinction, went in procession to congratulate this venerable person on the merits of his son, whom he had not seen

* See note, p. 579.

for more than 30 years, whom they declared "to have done honour to mankind by the brilliancy of his genius and whom Piedmont was proud to have produced, and France to possess as a citizen." The old man lived to the age of 95, and was sincerely regretted by his son.

Under the Consular and Imperial government, Lagrange was made a Senator, a Grand Officer of the Legion of Honour, a Count of the Empire, and a Knight Grand Cross of the Order of Reunion, in addition to the personal marks of friendship and intimacy which Bonaparte habitually conferred on him at the meetings of the Institute, and on other occasions.

He applied with so much zeal to the republication of the first part of his *Mechanics* in 1811, and of his *Analytical Functions* in 1813, that his health is supposed to have suffered from the fatigue; which, in conjunction with a predisposition, not uncommon in advanced life, may very possibly have been the immediate cause of a fit that attacked him in the beginning of the latter year. In the month of March he was subject to frequent returns of fainting, accompanied by some fever. On the 8th of April he had a last conversation with Lacépède, Monge, and Chaptal, all the parties being aware that it was to be the last. He felt the approach of death, but he declared that it was in that form neither painful nor even disagreeable. He spoke with proper gratitude of the favours he had received from Bonaparte, who afterwards provided very liberally for his widow and his brother. The interview lasted more than two hours, and though his memory often failed him with respect to names and dates, yet his language was correct and energetic. He survived this effort only two days, and died on the morning of the 10th April, 1813. He was buried at the Pantheon, or the church of St. Géneviève, and his friends Lacépède and Laplace paid the last honours to his memory in a funeral oration.

Lagrange was habitually of delicate health, and extremely temperate in his diet and mode of life, limiting his food almost entirely to vegetables, and taking his exercise very punctually in the open air. At the age of 56 he married the young and handsome Miss Lemonnier, who appears to have felt the splendour of his celebrity and the goodness of his heart, as affording

much more than a compensation for the great inequality of their ages. He was deeply sensible of her affectionate attachment, which he considered as the greatest happiness of his life, and on account of which alone he regretted its termination. He had no children, and he was perfectly contented to be without them. In the midst of the most brilliant societies he was generally absorbed in his own reflections; and especially when there was music, in which he delighted, not so much for any exquisite pleasure that he received from it, as because, after the first three or four bars, it regularly lulled him into a train of abstract thought, and he heard no more of the performance, except as a sort of accompaniment assisting the march of his most difficult investigations, which he thus pursued with comfort and convenience. He was less fond of the theatre, from which he often returned without knowing what piece had been represented. His manner in conversation was gentle and timid: he was more in the habit of interrogating than of giving his opinion, and his favourite expression was, "I don't know" . . . . He was not, however, easily induced to change his sentiments when they were once fixed, having generally adopted them upon mature consideration. As a writer, whenever any controversy occurred, he was always calm in defending himself, and respectful in speaking of his antagonists. Notwithstanding that his person was striking and characteristic as well as pleasing, he would never consent to have his portrait painted, thinking it unworthy of a man of intellectual excellence to wish to be remembered for the external form of his features. But a sketch of him was once obtained by stealth at a sitting of the Institute, and a mask of his face was taken after his death. His works bear witness, that for 54 years he occupied either the first or very nearly the first place among all the mathematicians of his age, and of all ages. "Of all the inventors," says Laplace, "who have the most contributed to the advancement of human knowledge, Newton and Lagrange appear to me to have possessed in the highest degree that happy tact, which enabled them to distinguish general principles among a multitude of objects enveloping them, and which is the true characteristic of scientific genius. This tact, in Lagrange, was united with a

singular elegance in the method of explaining the foundations of the most abstract truths of analysis." Lagrange was a great admirer of Euler, who perhaps excelled him in the adroitness with which he employed the most refined artifices of calculation, though his views and methods were less original and less powerful. D'Alembert was highly esteemed by Lagrange, as a man of abundant ingenuity and talent, though less accurate in his conclusions, and in his modes of reasoning, than either Euler or Newton. Newton he envied almost as much as he admired, for having found a system of the world in existence, and the principles of its modification not yet understood: but when it is remembered that the places of the heavenly bodies are now ascertained to seconds more nearly than they were to minutes during the life of Newton, it cannot be thought that Newton left too little for his successors to accomplish.

1. His first publication, at the age of 18, was, *A Letter to C. J. Fagnano*, 23rd June, 1754. It contains series for fluxions and fluents of different orders, somewhat resembling the binomial theorem of Newton.

2. The series of his papers in the *Miscellanies of Turin* is continued from 1759 to 1785. The first is *On Maxima and Minima. Misc. Taur.* I. 1759, p. 18. It is founded on the principles laid down by Maclaurin, and is illustrated by the case of the successive transmission of an impulse through a series of elastic bodies, comprehending the combination of a number of variable quantities. 3. *On an Equation of Finite Differences, and on the Theory of Recurring Series*, p. 33. The equation is resolved by an exponential integral, and the sum of the series is obtained by the principles of fluxions: the same mode of calculation is also applied to the laws of chance. 4. *Researches on the Nature and Propagation of Sound.* End of the volume. The investigations of Taylor and of Newton were true and correct as particular solutions only of the problems of chords and of undulations, though mistaken for general solutions, and as such successfully combated by Cramer, whose reasoning, though certainly too far extended, is here approved by Lagrange. Daniel Bernoulli very successfully defended them both, not only as particular solutions, but as capable of

being rendered universal by proper modifications and combinations. Euler had proposed a more general construction for the case of chords; D'Alembert insisted that this method required a limitation to figures exempt from angles and from abrupt changes of curvature, and Lagrange is inclined to admit his exceptions. But, after all, the question is merely a metaphysical refinement, since no abrupt changes can ever occur in the actual form of a chord; and a chord affording a harmonic of unlimited acuteness will approach without limit to a mathematical angle. The author begins, in this essay, with considering the motions of a finite number of bodies, and then proceeds to the affections of a fluid, which he reduces to the same equations as are applicable to the motions of chords, and these he integrates in D'Alembert's manner. He lastly examines the phenomena of the grave harmonics observed by Tartini, and explains them very satisfactorily from the analogy of the beats of discordant sounds. 5. *New Researches on Sound. Misc. Taur.* II. 1760-1. p. 11. The same subject is here continued, and extended to the divergence of sound, which had before been examined by Euler. The author now admits that there is no inconsistency in the demonstrations of Newton and Cramer, which deduce the same velocity from different laws of the supposed motion, since the velocity is really uniform in all cases. The oscillations of a heavy chain are computed, and some remarks are made in conclusion respecting the sounds of flutes. 6. *On the Maxima and Minima of Indefinite Integrals*, p. 173. This essay contains the foundation of the method of independent variations, which has excited so much attention for the universality of its application and the utility of its results. It was received with distinguished applause by Euler, as fulfilling his own wishes for the extension of a similar method; and it was Euler who more fully explained its principles, and gave it the name of the method of variations, which has since been generally applied to it. In fact, however, the foundation of the method had long before been laid by Leibnitz, under the name of differentiation from curve to curve; and he had proved that the process of integration, with respect to one kind of variation, might be applied to the differentials or fluxions

taken in another manner, without the necessity of first obtaining the fluent; and Euler had employed this consideration in treating of the geometrical properties of curves affording maxima or minima: but his method is less simple and less general than that of Lagrange, who first pointed out the universality of the principle, that the variation of the fluxion is equal to the fluxion of the variation, and showed its utility in many cases of such integrations, as leave the expression concerned still a fluxion of another kind: and in the mechanical application of the method, he made the fluxion of the ordinate of a vibrating chord represent its inclination to the axis at any given time, while its variation indicated its velocity or its change of place in successive intervals of time, and the fluxion of a revolving solid to relate to the magnitude of its different parts, while its variation depended on its rotatory velocity. The steps of the method are generally simple and easily understood, at least they may and ought to be rendered so; but the merit of the invention is not the less, because it admits of a very ready application, and because it might have occurred to a less distinguished mathematician; as indeed something nearly resembling it seems to have been employed by Fontaine in 1734, under the name of the fluxiodifferential calculus, in the investigation of a tautochronous curve. 7. It was particularly in demonstrating the law, which is called the law of the least action, that Lagrange completed the theory of variations, where Euler had felt its deficiency; and the application of the method to several *Mechanical Problems* constitutes the second part of the Memoir, p 106. The author takes occasion also to correct an error of D'Alembert, who had imagined that there was no necessity that the different strata of a given density, in a body like the earth, supposed to be in a state of fluidity, should all be level, which, however, is here shown to be a necessary consequence of D'Alembert's own equations. 8. *Addition to the Memoir on Sound*, p. 323; admitting the difficulty raised by D'Alembert, respecting the continuity of the figure of a chord, and acknowledging that the initial figure must not be supposed angular; while, in fact, as Mr. Fourier has lately demonstrated, and as had been remarked many years ago in this country, an infinite series of harmonic

curves may approach infinitely near to two right lines meeting in an angle. 9. *Problems relating to the Integral Calculus, Misc. Taur.* III. 1762-5, p. 179. A miscellaneous paper, containing remarks on the resolution of equations, containing fluxions of different orders; on some cases of the motions of fluids; on the vibrations of chords; on the properties of small oscillations in general; on loaded threads; on central forces; and on the theory of Jupiter and Saturn. 10. *An Arithmetical Problem, Misc. Taur.* IV. 1766-9, p. 44. This paper, dated at Berlin, contains a complete resolution of all equations of the second degree, having whole numbers for their roots; a problem, like most of those of Fermat, of more curiosity than utility, but well calculated to exercise the powers of minds like those of Euler, Lagrange, Legendre, and Gauss. The question, which is the particular subject of this paper, was proposed as a challenge by Fermat to his contemporaries in England, and correctly answered by Wallis, though without a very satisfactory demonstration. 11. *Integration of an Equation*, p. 98. A case in which the whole equation is integrable, though its parts, even when properly separated, are incapable of perfect integration. 12. *On the Method of Variations*, p. 163. In answer to Fontaine, and to Le Seur and Jacquier, who had attacked him in their Integral Calculus. 13. *On the Motion of a Body attracted by two fixed Centres*, p. 188, 216, including the effects of different supposed laws of attraction. 14. *On the Figure of Columns*, p. 123. This memoir contains an attempt to demonstrate that the cone is a more advantageous figure for the strength of a column than any conoid, and the cylinder than any cone. But the calculations are founded on the erroneous supposition, that the column must bend before it breaks; and, even upon this hypothesis, it appears possible to assign a stronger form than a cylinder, since the summit and the base of the cylinder must certainly contain some useless matter. 15. *On the Mean of a number of Observations*, p. 167; showing the advantage of taking the mean, from the theory of probabilities. 16. *On the Impulse of Fluids, Mém. Taur.* 1784-5, I. p. 75. The author observes, that this impulse will be measured by a column of twice the height due to the velocity, when the

whole impulse of the jet is received by an obstacle, but of the simple height, when a limited surface is exposed to the force of a larger stream. 17. *On the Integration of some Irrational Fluxions*, II. p. 218, involving the square root of an expression ascending to the fourth power of the variable quantity.

18. Some of the later of these papers are subsequent in date to those which are found in the *Memoirs of the Academy of Berlin;* but the order of enumeration is of little consequence. The first communication of Lagrange to the Academy, of which he was made director, is *On Tautochronous Curves, Mém. Berl.* 1765, p. 364. The paper is dated 1767; and it contains a completion of Fontaine's investigation of the subject. 19. *On the expected Transit of Venus*, 1766, p. 265. The author has here analytically investigated the curves of immersion and emersion for the different parts of the earth. But, as Mr. Delambre observes, in order to arrive at the very easy and tolerably accurate solution previously given by Delille and Lalande, he is obliged to employ in succession several elaborate expedients, founded on some very subtile principles, accompanied by various transformations of his ordinates, while, by a trigonometrical calculation of a few lines, we may obtain a more complete formula, comprehending even the terms which he has neglected, and which, although very small, are not absolutely insensible. At the same time, he has certainly applied his formula to the calculation of the parallax of the sun, in a very convenient manner, which had accidentally escaped both Delille and Lalande, though it follows readily from the trigonometrical calculation. 20. *On indeterminate Problems of the Second Degree*, 1767, p. 165. This is the first of a numerous series of papers, relating to this difficult branch of analysis, which, notwithstanding its practical inutility, has afforded sufficient scope for the exertion of talent, to give celebrity to the names of Diophantus and Fermat among the most ingenious of mathematicians. 21. *On Numerical Equations*, p. 311. This subject was also much cultivated by the author at a subsequent period: he here finds an equation for the differences of the roots, and exhibits the result in the form of a continued frac-

tion. 22. *Continuation of the Memoir on Numerical Equations*, 1768, p. 111. The method of continued fractions is still further improved. 23. *On the Resolution of indeterminate Problems in whole Numbers*, p. 181. 24. *On the Resolution of literal Equations by Series*, p. 251. The contents of these memoirs have been principally merged in the author's later productions. 25. *On the Force of Springs*, 1769, p. 167. It is demonstrated in this interesting paper, that the force of a hair-spring approaches to the law of a circular pendulum, the more nearly as its length is greater. 26. *On Kepler's Problem*, p. 204. An application of the methods explained in the last volume, especially of a very elegant formula for the reversion of series. 27. *On Elimination*, p. 303. A refined and general method of exterminating a quantity from an equation, which, however, is somewhat intricate, even in the simplest cases. 28. *Remarks on Isochronous Curves*, 1770, p. 97. Chiefly in answer to Fontaine, who had attacked him, and who had claimed the invention of the test of integrability of an expression containing several variable quantities. Lagrange observes, that he might very possibly have rediscovered it, but that it was published by Nicolas Bernoulli in 1720; by Fontaine, not till 1738. 29. *On Arithmetical Theorems*, p. 123, relating to the decomposition of a number into squares. 30. *On the Resolution of Equations*, p. 134. 31. *On a Theorem respecting prime Numbers*, 1771, p. 125. A demonstration of the property of prime numbers discovered by Mr. Wilson, and published by Waring; and of some other theorems of Waring. 32. *On Equations*, p. 138. In continuation. 33. *On a new Mode of Differentiation and Integration*, 1772, p. 185. The novelty consists in considering the characteristic of a fluxion as a quantity multiplying the letter to which it is prefixed, and inferring by induction that the result of the combinations obtained will in general remain unaltered by the supposition. The grounds of this method have been of late more fully explained by Arbogast and others. The results are here applied to interpolations, and to differences of various orders. 34. *On the Form of imaginary Roots*, p. 222. In general reducible to $A + \sqrt{-1}B$. 35. *On Astronomical Refractions*, p. 259; without any practical appli-

cations. 36. *On Equations of partial Differences*, p. 353; especially on finding multipliers to make them integrable. 37. *On undisturbed Rotation*, 1773, p. 85. A more direct method of investigation than that of Euler or D'Alembert; but without any new results. 38. *On the Attraction of Elliptic Spheroids*, p. 121. The author observes, that Maclaurin's prize essay is a masterpiece of geometry, comparable to the best works of Archimedes, though D'Alembert had once doubted the accuracy of some of his propositions. Thomas Simpson's was the first analytical solution of the problem, but it was indirect, and depending on series only. In this paper the method of demonstration only is varied. Legendre and Laplace subsequently continued the inquiry. "But Mr. Ivory," says Delambre, "has lately shown us, that a very simple consideration may in some cases supersede a multitude of calculations, and even afford us theorems to which the most prolix computations could scarcely have conducted us." 39. *On Triangular Pyramids*, p. 149. An analytical determination of the content, and of the figures that may be inscribed in the pyramids, when their six sides are given. 40. *Arithmetical Researches*, p. 265; on the integral roots of equations of the second degree. 41. *On particular Integrals*, 1774, p. 197. Laplace had already pointed out the occasional occurrence of integrals, not included in the general and direct expression obtained by the usual modes of integration. Such values are here deduced from the variation of the quantities originally considered as constant, which often affords us an equation of a different form, and leads to values not comprehended in the regular expression of the integral. 42. *On the Motions of the Nodes of the Planetary Orbits*, p. 276. Euler, Lalande, and Bailly had found some expressions for the temporary change of position of the nodes; the equations are here integrated, and the total changes determined 43. *On Recurring Series varying in two ways, or on partial Finite Differences*, 1775, p. 183. With an application to the theory of chances, upon Laplace's principles. 44. *On Spheroids*, p. 273. A demonstration of Maclaurin's theorem (*Fluxions*, Art. 653) concerning the attraction of a compressed spheroid or an amygdaloid; derived from the formulæ contained in the

former papers. 45. *Arithmetical Researches continued*, p. 323. Demonstrating some theorems of Fermat with which Euler had not succeeded: yet leaving others still unattempted. 46. *On the Mean Motions of the Planets*, 1776, p. 199. Showing that all their changes are periodical Laplace had detected an error in the author's reasoning when he attributed secular equations to the motions of Jupiter and Saturn, the expressions, containing the terms in question, being compensated by others which he had neglected. 47. *Cases of Spherical Trigonometry solved by Series*, p. 214. Without any apparent advantage. 48. *On Integration by continued Fractions*, p 236. Gives an example of the binomial theorem converted into a continued fraction, which, however, exhibits no particular elegance nor simplicity. 49. *On the Number of Imaginary Roots of Equations*, 1777, p. 111. Harriot was the father of the doctrine of equations. Newton made great improvements in it, but his rule remains imperfect with regard to the higher equations, even with the additions of Maclaurin and Campbell. In the present paper the theorem of Waring is demonstrated, without any material attempt to extend it. 50. *On the Diophantine Analysis*, p. 140. It is remarked that Fermat left all his propositions undemonstrated, except this theorem, that the sum of two biquadrate numbers can never be a square. 51. *On Escapements*, p. 173. An investigation of the best forms of pallets for the dead beat and the recoiling scapements. 52. *On determining the Orbits of Comets by three Observations*, 1778, p. 111, 124. The first part of this memoir is historical and critical, and the author allows due credit to the ingenuity of Newton's method: his own does not appear to have been of any practical utility. 53. *On the Theory of Telescopes*, p. 163. Comparing the general theorems of Cotes and Euler, and applying the method of recurring series to their demonstration; with a rule for determining the magnitude of the field. 54. *On the expression of the Time in a Conic Section*, p. 181. After Lambert, who determines it from the chord of the arc described, the sum of the revolving radii, and the great axis: the theorem is here analytically demonstrated. 55. *On particular Integrals*, 1779. p. 121. Examples from some mechanical curves. 56.

On Geographical Projections for Maps, p. 161, 186. The methods here proposed for the construction of maps have been found too intricate for adoption. 57. *On the Theory of the Libration of the Moon*, 1780, p. 203. In the prize essay on the moon's libration, the author had made the first application of the method of variations: the investigation is here continued, and it is observed that the moon cannot be of homogeneous matter, nor its form such as would afford equilibrium to a fluid covering it, since the effects of the ellipticity, so determined, would be much less perceptible than they are. 58. *Report on a Quadrature of the Circle. Hist. Ac. Berl.* 1781, p. 17. This paper only requires to be noticed as a specimen of the author's condescension. 59. *Theory of the Motion of Fluids, Mém.* p. 151. An application of D'Alembert's principles to the phenomena of running fluids, and to the motion of waves; but founded on an arbitrary assumption with respect to the depth affected by the waves. 60. *On the Secular Variations of the Elements of the Planets*, p. 199. The theory of perturbations is here examined by two methods, either comprehending the general form of the orbit, or regarding the local effects only. 61. *Report on a Mode of finding the Form of the Earth, Hist.* 1732, p. 35. A proposal of no value whatever. 62. *On the Secular Variations of the Planets, Mém.* p. 169. A continuation of the former memoir, with all the details of the application, and a determination of the change of the place of the ecliptic, together with a demonstration of the permanency of the general arrangement of the system, depending on the exemption of the mean distances from all variations not periodical, while the other elements are liable to greater alterations. 63. *On the Periodical Variations of the Planetary Motions*, 1783, p. 161. A sequel to the memoirs on the secular variations. 64. *Additions respecting the Secular Variations*, p. 191. Completing the examination, and extending it to the case of Jupiter and Saturn, which had before been investigated by Laplace. 65. *On the Correction of the Errors of Astronomical Approximations*, p. 224. The errors here considered arise from the employment of the powers of the arcs described in the equations concerned, these arcs increasing without limit: they may be

avoided by means of approximations founded on the supposition of the variation of the elements. Laplace had before employed a method still more refined. 66. *On a particular Mode of Approximation*, p. 279. Resembling that which Briggs employed for making logarithms. 67. *On a new Property of the Centre of Gravity*, p. 290. Relating to the mutual distances of the bodies. 68. *A direct and general Determination of the Motion of a Comet*, p. 296. In this third memoir the problem is reduced to equations of the 8th or 7th degree. 69. *Theory of the periodical Variations of the Planetary Motions*, 1784, p. 187. Continuation of the memoir of the preceding year, containing the independent variations of the eccentricities and inclinations, for the six principal planets; with a numerical application of the formulæ demonstrated in the first part. 70. *On the Integration of Equations of Linear Partial Differences*, 1785, p. 174. Entering into further details of the method laid down in a former paper, which is here applied to the problem of trajectories, a problem once proposed by Leibnitz as a trial of strength to Newton, who was not fully aware of the nature of the difficulty intended to be combated: it was, however, solved in England by Taylor, though indirectly. Nicolas Bernoulli and Hermann gave a more complete solution, and Euler added still more to the generality of the investigation. The author observes that the problem is a mere curiosity; there is, however, one case in which a trajectory of the kind here considered is actually applicable to a natural phenomenon of common occurrence, which is that of a wave diverging from a point in a gradually shelving shore; for the figure or direction of the collateral parts of such a wave may be shown to be the orthogonal trajectory cutting an infinite number of cycloids beginning at the given point. 71. *On the Motion of the Aphelia of the Planets*, 1786, i. A geometrical investigation, in the manner of Newton, intended as an appendix to the *Principia*. 72. *On the Theory of Sound and Waves*. This paper is also intended to complete the demonstrations contained in the same work. The volume, in which both these interesting memoirs appear, seems to have been published out of the regular order, from some circumstances connected with the death of Frederic; and it is

wanting in many of the British libraries. 73. *Note accompanying a Memoir of Duval le Roi*, 1786-7, p. 253. On the secular equations of the Georgian planet. 74. *On a Question relating to Annuities*, 1792-3, p. 235. The case of an annuity supposed to commence after a death, and to cease at a given age. 75. *Additions to former Memoirs*, p. 247. On recurring series (n. 43); on elliptic spheroids (n. 38); on interpolations, in Mouton's manner, comprehending the inequality of the distances of the observations; on the secular equation of the moon (n. 64). After Laplace's great discovery of the cause of the secular acceleration, Lagrange found that it might have been easily deduced from his own calculations, almost in the same form, if he had not accidentally neglected the application, from having assured himself, in 1783, that the results of a similar computation were nearly insensible in the case of Jupiter and Saturn. It was in 1787 that the discovery of Laplace was announced. The acceleration here computed is 10″,5 for the first century after 1800. Mayer found it 9″ from a comparison of observations. 76. *On a general Law of Optics*, 1803, Math. p. 3. A demonstration of the foundation of the method long used by English opticians for determining the magnifying powers of telescopes of all kinds, which form an image of the object-glass beyond the eye-glass, by measuring the diameter of that image. The author hazards, in this paper, the very singular assertion, that the illumination of the object must be the same in all telescopes whatever, notwithstanding the common opinion, that it depends on the magnitude of the object-glass; and his reasoning would be correct, if the pupil of the eye were always less than the image of the object-glass in question; since, as he observes, the density of the light in this image is always inversely as the magnifying power: but he forgets to consider that the illumination on the retina, when the whole pencil is taken in, is in the joint ratio of the density and the extent; a consideration which justifies the common opinion on this subject, and shows that a most profound mathematician may be grievously mistaken in his conclusions, if he proceeds to calculate upon erroneous grounds. It deserves, however, to be remembered, that the brightness of any given angular

portion of a magnified image must always be somewhat less than that of an equal portion of the object seen by the naked eye: because it can be no greater if the pencil fills the pupil, and will be less in proportion as the pencil is smaller than the pupil, besides the unavoidable loss of light at the refracting surfaces.

77. The later works of Lagrange have principally been published at Paris, and most of them in the various collections of the Academy. The earliest of these are the prize memoirs, and first the essay *On the Libration of the Moon*, which obtained the prize in 1764. *Ac. Par.* Prix IX. 1772. It is in this memoir that the method of variations was first practically applied to a mechanical problem. 78. *On the Inequalities of the Satellites of Jupiter*, in 1776. Including the consideration of their mutual perturbations, and consequently a case of the problem of six bodies. The author never resumed the subject, but its investigation was completed by Laplace. 79. *A new Method of solving the Problem of Three Bodies*, in 1772. 80. *On the Secular Equation of the Moon*, in 1774. *M. Sav. Etr.* VII. for 1773. An unsuccessful attempt, with conjectures respecting the existence of a resisting medium, and even doubts of the accuracy of the foundation of Halley's discovery. 81. A prize memoir, *On the Perturbation of Comets passing near Planets. M. Sav. Etr.* X. Par. 1785, p. 65. Finding the path directly, without regard to the conic sections, and employing three different modes of computation for the different parts of the orbit. 82. *On forming Tables by Observations only, Mém Ac. Par.* 1772, i. p. 513. The method of recurring series is principally employed, and the author observes that the problem is more useful than difficult, giving an experimental example in the equation of time, for which he obtains from the results of the tables an expression very near the truth. Delambre remarks that this is only a continuance of the system adopted by Ptolemy and the other ancient astronomers, showing what we might have done in a circuitous manner by pure mathematics, if Newton and the laws of gravity had not existed; and he thinks the paper only valuable as a specimen of Lagrange's talent for overcoming difficulties, which he might

more easily have avoided. 83. *On the Nodes and Inclinations of the Planetary Orbits*, 1774, p. 97. With details of the calculations for all the planets. 84. *On the Variation of the Elements of the Planets. Mém. Math. Inst.* 1808, p. 1. The object of this paper is to show, as Poisson had done before, that all the changes of the system are periodical. The method is more general, but less simple than that of Laplace, who first discovered the principle by induction. The lunar acceleration is given as an example. Mr. Poisson has extended his calculations to quantities of the second order, which do not enter into Lagrange's investigations. 85. *On the Variation of Independent Quantities in general in Mechanical Problems*, p. 257. The author observes that many of the modern improvements of mathematics depend on the doing away the distinction between constant and variable quantities, which was so valuable when it was first enforced by Descartes. 86. *A second Memoir on the Variation of Independent Quantities*, 1809, p. 343. Simplifying the general application of the doctrine.

87. *Lectures on Arithmetic and Algebra. Séances des Ecoles Normales*, year III., 1794-5. The first lecture relates to the elements of arithmetic, the second to the lower orders of equations, and the third to the higher. All these lectures, under the name of conversations, were taken down in short-hand by some of the students, and afterwards corrected by the professors.

88. *An Essay on Political Arithmetic.* Rœderer, *Collection de divers Ouvrages.* Paris, an IV. 1795-6.

89. In the *Journal de l'Ecole Polytechnique*, we find an *Essay on Numerical Analysis, and on the Transformation of Fractions*, Vol. II. 1798, p. 95. It contains the elementary theory of continued fractions, and the mode of reducing them. 90. *On the Principle of Virtual Velocities*, p. 115. Chiefly relating to pulleys. 91. *On the Object of the Theory of Analytical Functions*, VI. 1800, p. 232. A detailed explanation of the grounds of the theory laid down in the separate publication on this subject. 92. *Analysis of Spherical Triangles*, p. 270. Giving all their essential properties in a concise form. 93. *Lectures on the Calculus of Functions*, XII. 1804. Published also separately in 8vo. A commentary on the theory of func-

tions, and a supplement to it, contained in twenty lectures. 94. *Two more Lectures*, XIV. 1808; at the end, chiefly relating to the method of variations. 95. *On a Difficulty respecting the Attraction of Spheroids*, VIII. xv. Remarks which may serve as a commentary on a passage of the *Mécanique Céleste.*

96. *On the Origin of Comets. Connoissance des Tems*, 1814. 97. *On the Calculation of Eclipses, as affected by Parallax*, 1817. From the *Berlin Almanac* for 1782. This memoir, as Delambre observes, is singularly attractive to a person previously unacquainted with the methods which are employed; but though the formulæ first introduced are direct and rigorously accurate, the whole investigation ends in an approximation which wants both these properties.

98. The most important of all the works of Lagrange are those which have appeared in separate volumes; and among these we may reckon his *Additions à l'Algèbre d'Euler*. 8. Lyons, 1774, Vol. II. German, 1796. English, 1797. They relate chiefly to continued fractions, and to indeterminate problems, and constitute the most valuable part of the whole work, which, in its abridged form at least, is far inferior to Maclaurin's *Algebra.* 99. *Mécanique analytique,* 4. Par. 1788, 2nd edit. Vol. I. 1811, Vol. II. by Prony, Lacroix, and Binet, 1815. This work exhibits a uniform and elegant system of mechanical problems, deduced from the simple principle of virtual velocities, which was well known to former authors, but never so extensively applied. It has been remarked, that many parts of it may be read with advantage, even by those who are not competent to enter into any of the computations, exhibiting such a history of the progress of the science, as could only have been sketched by a master. The new edition, begun at the age of 75, comprehends all the improvements contained in the author's later memoirs on various subjects. 100. *Théorie des Fonctions analytiques*, 4. Par. 1797, 1813. The abstract theory of analytical functions has been very fashionable among modern mathematicians; but the improvements which it contains are chiefly of a metaphysical nature, if they can with propriety be called improvements. The notation is less simple than that which is in common use, and has been abandoned by the author in some

republications of the works in which he had at first employed it. The calulations, too, are often more intricate than others which afford the same conclusions. 101. *Résolution des Equations numériques*, 4. Par. 1798, 1808. The refined and abstruse speculations, contained in this volume, are more calculated to promote the advancement of the abstract science of quantity, than to be applied, as the title would seem to denote, to the purposes of numerical computation. The methods investigated are in general laborious and complicated, though instructive, and capable of extensive application; and for equations, of which all the roots are real, the author himself recommends Mr. Budan's method as preferable to his own. The second edition contains a number of very interesting notes, which are full of ingenuity and novelty.

The author of so immense a series of laborious investigations must certainly have been a most extraordinary man. He had acquired the character of an illustrious mathematician almost in his boyish years; and he continued to apply the force of his powerful mind, for more than half a century, to the almost uninterrupted pursuit of his favourite sciences. It seems, however, that his earliest were also his greatest successes; and all that followed was as little as could well be expected, from a continued employment of the means which he possessed at the beginning; for, in fact, the whole taken together appears to bear a stronger character of great industry, than of great sagacity or talent.*

"It was formerly usual," says Delambre, "for mathematicians to inquire, in every investigation, for some general

* Few mathematicians will be disposed to acquiesce in this judgment or in many of the observations which follow. Of all the great analysts, Lagrange was the most remarkable for combining the highest generalizations with the greatest simplicity and elegance of form. It was no sufficient objection to him to assert that he frequently applied the general methods of analysis to the solution of questions which could be much more easily effected by less general views or by geometrical or other means; his object was to show that the instrument which he employed was equally capable of special and general applications. As a critical expositor likewise of different methods of treating the same subjects, with a view of showing their relations to each other and the true principles upon which they depend, he is unrivalled; and though it may be quite true that he is inferior to Laplace in the extent and continuity of his applications of analysis to physical astronomy and other departments of natural philosophy, yet his invention of the method of the variation of parameters and his proof of the stability of the planetary system are not surpassed in importance or originality by any production of his illustrious contemporary.—*Note by the Editor.*

considerations, which might be capable of simplifying it, or of reducing it to a problem already resolved, and to endeavour by these means either to abridge the calculation, or sometimes to supersede it altogether. But since the discovery of the infinitesimal calculus, the facility and universality of this method, which often renders the possession of ANY TALENT IN THE CALCULATOR WHOLLY UNNECESSARY, has made it more usual for mathematicians to direct their chief attention to the perfection of this all powerful instrument. But at the present day, when researches of this kind appear to be completely exhausted by the labours of Euler, Lagrange, and their industrious contemporaries, it might perhaps be more advisable to return to the ancient method, and to follow the example of" Newton, surely, and of " Daniel Bernoulli, who, as Condorcet observes, was entitled to the praise of moderation in the introduction of his calculations Lagrange was in the habit of employing his sublime talents in a different manner: he liked to make every thing dependent on his analysis, though, in some instances, he united both methods in the highest degree, as his invention of the calculus of variations bears witness. His reducing the theory of sound to that of the vibration of chords, is a specimen of a very ingenious simplification; as well as his mode of computing the planetary motions by the variation of the elements of their orbits, which is also applicable to all other cases of the operation of small disturbing forces. But it must be confessed, on the other hand, that he has sometimes created difficulties where none existed, by applying his profound and ingenious methods to the solution of elementary problems, which may be obtained from a construction of the simplest kind; and the powerful agents which he employs, on many trifling occasions, remind us only of the man in the fable, who came to borrow the club of Hercules, and the thunder of Jupiter, for the purpose of destroying a flea;" or of the modern mathematician, who, without any fable, or any figure of rhetoric, proposed to adjust a standard measure, by placing it at a distance, and viewing it with a good telescope. The habit of relying too confidently on calculation, and too little on common sense, will perhaps account for the mistakes of Lagrange, which have been

already noticed, respecting the forms of columns, and the illumination of optical instruments: nor are they the only instances of the kind which may be produced from the modern history of the sciences. It seems, indeed, as if mathematical learning were the *euthanasia* of physical talent; and, unless Great Britain can succeed in stemming the torrent, and in checking the useless accumulation of weighty materials, the fabric of science will sink in a few ages under its own insupportable bulk. A splendid example has already been displayed by the author of the article ATTRACTION in this *Supplement*;* and to do justice to our neighbours, it must be allowed that they have received the boon with due gratitude, and acknowledged it by merited applause: "All the analytical difficulties of the problem," say Legendre and Delambre (*M. Inst.* 1812), "vanish at once before this method; and a theory, which before required the most abstruse analysis, may now be explained, in its whole extent, by considerations perfectly elementary." It is, indeed, only when a subject is so simplified, that the investigation can be considered as complete, since we are never so sure that we understand the process of nature, as when we can trace at once in our minds all the steps by which that process is conducted. It is not without some reason that a similar disposition, to revel in the luxury of mathematical sports, has been sometimes objected to Laplace, a man of equal analytical acquirements with Lagrange, but possessed apparently of greater sagacity, and certainly more successful in his application of mathematics to physical researches, although he also seems, on some occasions, to have suffered his habits of abstract reasoning to lead away his attention from the true conditions of the problem; particularly in his first supplement respecting capillary attraction, which concludes with an equation so erroneous, that he has been obliged to abandon it in silence. (See the article COHESION in this Work, vol. i., No. XX.) Another instance of ill applied computation has been noticed in the article CHROMATICS (vol. i., No. XV.); when Laplace attempted to deduce the laws of extraordinary refraction from the principle of the minimum of action, he seems to have forgotten that the demonstration of that principle,

* Mr. Ivory.—*Note by the Editor.*

in his own great work, rests *expressly* on a condition which is here wanting, that "the forces concerned must be functions of the distances," and of course independent of the directions. These imperfections, however, would not deserve to be noticed as materially affecting the general merits either of Lagrange or of Laplace, but they may be considered as accidents, which ought to warn us against relying too implicitly on authority, however high, when it appears to militate against clear simple reasoning and sound common sense.

No. LXXX.

LIFE OF FERMAT.

Peter de Fermat, equally celebrated as a restorer of ancient mathematics, and an original author of modern improvements, was born in 1590.

His public life was occupied by the active duties attached to the situation of a Counsellor of the Parliament of Toulouse, in which he was distinguished both for legal knowledge and for strict integrity of conduct. Besides the sciences, which were the principal objects of his private studies, he was an accomplished scholar, an excellent linguist, and even a respectable poet.

His *Opera Mathematica* were published at Toulouse, in two volumes folio, 1670 and 1679; they are now become very scarce. The first contains the *Arithmetic of Diophantus*, illustrated by a commentary, and enlarged by a multitude of additional propositions.* In the second we find a *Method for the Quadrature of Parabolas* of all kinds, and a *Treatise on Maxima and Minima, on Tangents, and on Centres of Gravity;* containing the same solutions of a variety of problems as were afterwards incorporated into the more extensive method of fluxions by Newton and Leibnitz; and securing to their author, in common with Cavalleri, Roberval, Descartes, Wallis, Barrow, and Sluse, an ample share of the glory of having immediately prepared the way for the gigantic steps of those illustrious philosophers. The same volume contains also several other treatises on *Geometric Loci*, or *Spherical Tangencies*, and on the *Rectification of Curves,* besides a restoration of *Apollonius's Plane Loci;* together with the author's correspondence, addressed to *Descartes*, *Pascal*, *Roberval*, *Huygens*, and others.

It was too much Fermat's custom to leave his most impor-

* Supra, pp. 568, 569.

tant propositions wholly undemonstrated; sometimes, perhaps, because he may have obtained them rather by induction than by a connected train of reasoning; and, in other cases, for the purpose of proposing them as a trial of strength to his contemporaries. The deficiency, however, has in many instances been supplied by the elaborate investigations of Euler and Lagrange, who have thought it no degradation to their refined talents to go back a century in search of these elegant intricacies, which appeared to require further illustration. It happened not uncommonly, that the want of a more explicit statement of the grounds of his discoveries deprived Fermat, in the opinion of his rivals, of the credit justly due to him for accuracy and originality. It was thus that Descartes attempted to correct his method of maxima and minima, and could never be persuaded that Fermat's first propositions on the subject were unexceptionable. Fermat was however enabled to pursue his favourite studies with less interruption than Descartes; and the products of his labour were proportionate, as Lacroix remarks, to the opportunities that he enjoyed, as well as to the talents that he possessed.

There is a very ingenious proposition of Fermat, which deserves to be particularly noticed, on account of the discussion that it has lately excited among mathematical philosophers. He has demonstrated, that the true law of the refraction of light may be deduced from the principle, that it describes that path, by which it can arrive in the shortest possible time from any one point of its track to another; on the supposition, however, that the velocity of light is inversely proportional to the refractive density of the medium; and the same phenomena of refraction have been shown, by Maupertuis, to be deducible, upon the opposite supposition with respect to the velocities, from the law of the minimum of action, considering the action as the product of the space described into the velocity. But the law of Fermat is actually a step in the process of nature, according to the conditions of the system to which it belongs in its original form; while that of Maupertuis is at most only an interesting commentary on the operation of an accelerating force. It was Newton that showed the necessary connection

between the action of such a force and the actual law of refraction; demonstrating that all the phenomena might be derived from the effect of a constant attraction, perpendicular to the surface of the medium; and except in conjunction with such a force, the law of Maupertuis would even lead to a false result. For if we supposed a medium acting on a ray of light with two variable forces, one perpendicular to the surface, and the other parallel to it, we might easily combine them in such a manner as to obtain a constant velocity within the medium, but the refraction would be very different from that which is observed, though the law of Maupertuis would indicate no difference; so that the law must be here applied with the tacit condition, that the refractive force is perpendicular to the surface. In M. Laplace's theory of extraordinary refraction, on the contrary, the tacit condition is, that the force must *not* be perpendicular to the surface: so that this theory not only requires the gratuitous assumption of a different velocity for every different obliquity, which is made an express postulate, but also the implicit admission of the existence of a force, determinate in direction and in magnitude, by which that velocity is modified, and without which the law of Maupertuis would cease to be applicable. It may indeed be said, that the supposition of a medium exhibiting unequal velocities, and attracting the light perpendicularly, is unnatural; and that the law is the more valuable for not being applicable to it: but a mathematical equation is true even with respect to impossible quantities; and a physical law, however useful it may be, requires physical proof; and it will not be asserted that the law of Maupertuis has been or can be established, by physical evidence sufficiently extensive to render it universal.*

Our author died in 1664, or the beginning of 1665, at the advanced age of 74. He left a son, Samuel de Fermat, who was a man of some learning, and published translations of several Greek authors.

* See page 581, No. LXXIX.

No. LXXXI.

LIFE OF DOLLOND.

JOHN DOLLOND, a practical and theoretical optician of the highest celebrity; the discoverer of the laws of dispersion of light, and inventor of the achromatic telescope; descended from a family of French refugees, was born in London 10th June, 1706.

His first destination was the manufacture that afforded employment to the greater part of the French colony established in Spitalfields; and he passed some of his earlier years in the mechanical labour of a silk weaver. He was, however, always attached to the mathematics and to natural philosophy; and he even extended his studies to the outlines of anatomy and of scholastic divinity: and in the pursuit of these objects he found himself obliged to acquire a competent knowledge of the Latin and Greek languages: a task which was much facilitated to him by the possession of a memory no less retentive, than his observation was accurate, and his reasoning correct. He married early; and he continued in his first occupation till he had established his eldest son, Peter Dollond, who inherited his own tastes, as an optical instrument maker; and the success of the undertaking was such, as to induce him, in 1752, to leave his own business, and to enter into partnership with his son in Vine Court.

These arrangements having taken place, it was not long before Mr. Dollond communicated to the Royal Society some of the results of the application of his inventive powers to his new pursuits: and Mr. Short, who then enjoyed the highest reputation as an optician, paid him the compliment of bringing them forward at the Society under the auspices of his name.

1. *A Letter to Mr. James Short, F.R.S., concerning an Improvement of Refracting Telescopes. Phil. Trans.* 1753,

p. 103. The author here describes a telescope with six glasses, as calculated for correcting, either wholly or in great measure, the errors of refraction arising from the dispersion of the different colours, as well as from the spherical form of the surfaces of the eye-glasses: appealing to the superiority of the telescopes, which he had thus constructed, above those which had before been in use: but he reserves a more ample detail of the theory for a future occasion; which, however, does not appear to have presented itself, the improvement having been superseded by others incomparably more important.

2. *A Letter to James Short, A.M., F.R.S., concerning a mistake in Mr. Euler's Theorem for correcting the Aberration in the Object Glasses of Refracting Telescopes*; read 23rd November, 1752; together with an introductory letter of Mr. Short, in which Euler's calculations are somewhat too categorically condemned, and with Euler's answers to Short and Dollond. *Phil. Trans.* 1753. p. 287. It is remarkable, with what profound respect the experiments of Newton are treated in Mr. Dollond's letter: "It is somewhat strange," he says, "that any body now-a-days should attempt to do that which so long ago has been demonstrated impossible:" but although the investigation of truth was perhaps in this instance retarded, yet its ultimate discovery was not prevented by a just deference to a high authority. Euler was, however, certainly right in considering the law which he had assumed as sufficiently compatible with the results of Newton's experiments; although he was much mistaken in his conjectures respecting the achromatic properties of the eye.

3. *A Description of a Contrivance for Measuring Small Angles. Phil. Trans.* 1753, p. 178. This apparatus consists of a divided object-glass, with a scale for determining the distance of the images, by measuring the linear displacement of the two portions of the glass; which subtends the same angle from the focus of parallel rays, as the actual distance of the images does from the object-glass. The apparatus is recommended as particularly calculated to be applied to a reflecting telescope, and was afterwards adapted by Mr. Peter Dollond to the improved achromatic telescopes. Mr. Savery and Mr.

Bouguer had before used two separate lenses in a manner nearly similar; but the employment of a single glass divided affords a much more convenient arrangement.

4. *An Explanation of an Instrument for Measuring Small Angles. Phil. Trans.* 1754 p. 551. This paper contains a more detailed theory of the divided object glass micrometer, and a testimony of its utility from Mr. Short, founded on actual experiments.

5. *An Account of some Experiments concerning the Different Refrangibility of Light. Phil. Trans.* 1758. p. 733. We have here the important results of a series of accurate experiments, by which the author had undertaken to investigate the foundations of the Newtonian theory of refraction; though he began them without any hope of a success so brilliant as that which ultimately crowned his labours.

It was in the beginning of 1757 that Mr. Dollond made the decisive experiment of putting a common prism of glass into a prismatic vessel of water, and varying the angle of the vessel till the mean refraction of the glass was compensated; when he found that the colours were by no means destroyed, as they were supposed to have been in a similar experiment related by Newton; for the remaining dispersion was nearly as great as that of a prism of glass of half the refracting angle. Mr. Dollond then employed a thinner wedge of glass, and found that the image was colourless when the refraction of the water was about one-fourth greater than that of the glass. He next attempted to make compound object-glasses by inclosing water between two lenses; but in this arrangement he found great inconvenience from the spherical aberration. So that he was obliged to try the effects of different kinds of glass, and he fortunately discovered that the refractions of flint and crown glass were extremely convenient for his purpose, the image afforded by them being colourless, when the angles were to each other nearly as 2 to 3; and hence he inferred that a concave lens of crown glass, and a convex one of flint, would produce a colourless image when their focal distances were in the same proportion. The spherical aberration, where the curvature was so considerable, still produced some inconvenience

but having four surfaces capable of variation, he was enabled to make the aberrations of the two lenses equal; and since they were in opposite directions, they thus corrected each other. All these arrangements required great accuracy of execution for their complete success; but, in the hands of the inventor, they produced the most admirable instruments; and he was singularly fortunate in obtaining a quantity of glass of more uniform density than has been since manufactured on so large a scale. He afterwards made some small Galilean telescopes with triple object-glasses, and Mr. Peter Dollond applied this construction to the longer telescopes, with compound eye pieces, the alteration rendering the spherical aberration still more manageable.

The merits of Mr. Dollond's inventions were promptly acknowledged on the part of the Royal Society, by the adjudication of the Copleian medal for the year. In 1761 he was appointed Optician to the King, and was elected a Fellow of the Royal Society; a distinction which is often obtained on easy terms by those whose situation in life exempts them from the suspicion of seeking it for any purpose degrading to science; but which is generally an object of considerable ambition to persons of mechanical or commercial occupations.

A considerable share of the credit due to Mr. Dollond's discoveries has been very erroneously attributed, by some late historians and biographers on the Continent, to Leonard Euler, a mathematician who, most assuredly, has little need of the appropriation of the merits of others to establish his claim to immortality. But in fact the only idea of Euler, that could be said to have furnished any hint to Mr. Dollond, has been shown by the calculations of Dr. Maskelyne, and by the experiments of Dr. Thomas Young,* and Dr. Wollaston, to have been completely erroneous; nor did Euler even admit the accuracy of Mr. Dollond's conclusions, after his discovery was made, without considerable hesitation and scepticism. Mr. Klingenstierna had simply expressed a doubt with respect to the result of Newton's experiments, though he by no means suspected the extent of the error. Mr. Peter Dollond has sufficiently vindi-

* See supra, vol. i., p. 148.

cated his father's claim to complete originality, in a paper read to the Royal Society in 1789; he has also suggested an explanation of the origin of Newton's mistake, by stating that there exists a kind of Venetian glass, of which the dispersive power little exceeds that of water, while its specific gravity nearly approaches to 2.58, which is assigned by Newton to glass in general; and it certainly seems more probable that some such circumstance as this was the cause of the error, than that Newton should, as some have suspected, have mixed acetate of lead with the water which he used, for an experiment which was so much more likely to be satisfactory without it.

Mr. Dollond's appearance was somewhat stern, and his language was impressive, but his manners were cheerful and affable. He was in the habit of attending regularly, with his family, the service of the French Protestant church. He constantly sought his chief amusement in objects connected with the study of those sciences which he had so much contributed to improve. Perhaps, indeed, he pursued them with an application somewhat too intense: for on the 30th of November, 1761, as he was reading a new work of Clairaut on the theory of the moon, which had occupied his whole attention for several hours, he had an attack of apoplexy, which shortly became fatal. He left two sons and three daughters. His sons succeeded to his business; and the younger dying a few years after, his place was filled by a nephew, who assumed the family name, and who still conducts the establishment with undiminished respectability and success.

No. LXXXII.

LIFE OF MALUS.

Stephen Lewis Malus, the discoverer of the Laws of the Polarisation of Light, born at Paris, 23rd June, 1775, was the son of Anne Lewis Malus du Mitry, and of Louisa Charlotte Desboves, his wife.

His father had a place in the Treasury of France, and gave him an excellent education at home in mathematics and in the fine arts; as well as in classical literature, with which he rendered himself so familiar, as to retain many passages of the *Iliad* in memory throughout his life. At seventeen he was admitted, after a severe examination, as a pupil of the School of Military Engineers; and about the same time he amused himself with writing a regular tragedy in verse on the death of Cato. He soon distinguished himself in his military studies, and he was about to obtain a commission as an officer, when an order of the minister Bouchotte imputed to him the offence of being a suspected person, probably on account of the situation held by his father, and he was dismissed from the school. He was then obliged to enter the army as a private soldier in the 5th battalion of Paris, and he was employed in this capacity on the fortifications of Dunkirk. Here he was soon distinguished by Mr. Lepère, the director of the works, as superior to his accidental situation; and he was selected as one of the young men who were to constitute the members of the *Ecole Polytechnique*, then to be established upon the recommendation and under the direction of Monge, who immediately chose him, from a previous knowledge of his merit, as one of the twenty that were to be made instructors of the rest. This body constituted, at that moment, the only refuge of the sciences in France, and the enthusiasm of its members was proportionate to the advantages which they enjoyed, and to the importance of

the trust committed to them. In the three years which he passed in this institution, he was much employed, among other applications of the higher geometry, in pursuing the mathematical theory of optics, a department of science in which he was afterwards so eminently to distinguish himself by experimental discoveries. He was then, however, obliged to abandon for a time the pursuit of scientific investigations, and he was admitted into the corps of engineers, with the seniority of his former rank in the school. He served in the army of the Sambre and Meuse ; he was present at the passage of the Rhine in 1797, and at the affairs of "Ukratz" and Altenkirch. While he was in Germany, he formed an engagement with Miss Koch, the daughter of the Chancellor of the University of Giessen, and he was on the point of marrying her, when he was obliged to join the Egyptian expedition. He was present in that campaign, at the battles of Chebreïs, and of the Pyramids ; he was at the affair of Sabish, at the siege of El Arish, and at that of Jaffa. After the surrender of that place, he was employed in the repairs of the fortifications, and in the establishment of military hospitals. Here he was attacked by the plague, and fortunately recovered from it without any medical assistance. He was then sent to fortify Damietta ; he was afterwards at the battle of Heliopolis, at the affair of Ceraim, and at the siege of Cairo. After the capitulation with the English, he embarked on board of the transport Castor, and arrived in France the 26th October, 1801. His health was exhausted, and his spirits were broken, by fatigue and anxiety ; but his attachment to his betrothed bride was undiminished, and he hastened to Germany to fulfil his engagement : his fidelity was rewarded, during the eleven years that he survived, by the most constant and affectionate attention on the part of his wife ; and she died a year or two after him, a victim to the same disease which had been fatal to her husband.

He had, however, enough of strength and vigour of constitution remaining, to enable him, besides the official superintendence of the works carrying on at Antwerp and at Strasburg, to pursue the study of his favourite sciences ; and upon occasion

of a prize question, proposed by the Institute, he undertook the investigation of the extraordinary refraction of Iceland crystal, which the experiments of Dr. Wollaston had lately shown to agree very accurately with the laws laid down by Huygens; and besides completely confirming all Dr. Wollaston's results, he had the good fortune greatly to extend the Huygenian discovery of the peculiar modification of light produced by the action of such crystals, which Newton had distinguished by the name Polarity, and which Malus now found to be produced in a variety of circumstances, independently of the action of crystallized bodies. It seems natural to suppose that the investigation of the laws of the internal reflection of light, at the second surface of the crystals, must have led him to the discovery of the effects of oblique reflection in other circumstances; but according to Biot, there was more of accident in his actual progress; for he informs us that Malus had been looking through a piece of crystal at the image of the sun, reflected from the windows of the Luxembourg, to the house in the Rue d'Enfer, where he lived, and that he was much surprised to find one of the double images disappear in a certain position of the crystal; although the next day, at a different hour, he could no longer observe the phenomenon, from the alteration of the angle of incidence.

The merit of his discovery was soon acknowledged by his election as a Member of the Institute, as well as by the adjudication of a Biennial Medal from the Royal Society of London, on the foundation of Count Rumford. It has been thought creditable to the Royal Society to have conferred this distinction in the time of a war between the two countries; but if any credit were due for only doing justice conscientiously, it would attach, on this occasion, to those members of the Council, who saw their own optical speculations in great danger from the new mass of evidence, which appeared likely to overthrow them, at least in the public opinion, and who were still the most active in offering this tribute of applause to the more fortunate labours of a rival.

Nor was the remuneration of Malus confined to empty honours only; he obtained promotion, from the liberality of the

French government, *in his own profession* AS A MILITARY MAN; and this not for services performed in the field, nor even in a difficult and dangerous expedition to unknown regions, but for experiments made with safety and tranquillity in his own closet. That government had not carried the refined principle of the division of labour so far, as to have resolved that all public encouragement should be limited to the precise department in which a public service had been performed; and a mark of distinction, which a gentleman could accept without degradation, was not deemed an incommensurate remuneration for a discovery in abstract science. Such a refinement, which has been practically introduced in our own matchless country, might appear, to a man who had a heart, something worse than sordid; he might fancy that a great nation, as well as a great individual, should treat its dependents, "not according to their deserts, but after its own honour and dignity:" if, however, a person in office happened to have any thing like a heart about him, the outcry of an indiscriminating opposition would soon teach him to silence its dictates.

1. Mr. Malus's first publication appears to have been a paper *On an Unknown Branch of the Nile*, in the first volume of the *Décade Egyptienne*. 2. He presented to the Institute a mathematical *Traité d'Optique*, before the completion of his experiments on double refraction; it was published in the *Mémoires présentés à l'Institut*, II. 4. Paris, 1810. 3. His more important discoveries were first made known in the second volume of the *Mémoires d'Arcueil*, 8. Paris, 1809; and again, 4. in the *Theory of Double Refraction*, *Mém. prés. à l'Inst.* II., a paper which obtained a prize the 2nd January, 1810. 5. In a short *Essay on the Measurement of the Refractive Force of Opaque Bodies*, contained in the same volume, he employs the method, before made known by Dr. Wollaston, for conducting the experiment, and computes the forces concerned upon the Newtonian hypothesis; applied, however, in a manner somewhat arbitrary to the circumstances of the problem. 6. *Remarks on some new Optical Phenomena*, *Mém. Inst. Sc.* 1810, p. 105, Paris, 1814, read 11th March, 1811. This paper is principally intended to prove that two portions of light are always

polarised together in opposite directions, and that no part of the light concerned is destroyed, "as Dr. Young had been inclined to suspect;" the author found that light transmitted obliquely through a number of parallel glasses at a proper angle, becomes at last completely polarised. Mr. Arago had discovered a case which appeared to be an exception to the general law of the polarisation of transmitted light, but it was afterwards readily explained from the theory of the production of colours by interference, as applied to transmitted light. A letter, containing the substance of this paper, was published in Thomson's *Annals*, III. 257, April, 1814, on occasion of some discoveries of Dr. Brewster, which had been supposed to be wholly new. 7. *On Phenomena accompanying Refraction and Reflection*, p. 112; read 27th May, showing the universality of polarisation at a proper angle, and examining the effect of a metallic surface. 8. *On the Axis of Refraction of Crystals*, p. 142; describing an apparatus for finding the properties of bodies with respect to polarised light, applied to the determination of the axis of crystals, and to the examination of the structure of organised bodies, which appear in general to have certain axes of polarisation, as well as those which are manifestly crystallized.

The zeal and energy of Malus supported him to the last, not only in the continuance of these interesting investigations, but also in his duties as an examiner at the *Ecole Polytechnique*. He died, 24th February, 1812, universally regretted by the lovers of science in all countries, and deeply lamented by his colleagues, who said of him, as Newton did of Cotes, that if his life had been prolonged, we should at last "have known something" of the laws of nature.

No. LXXXIII.

LIFE OF LALANDE.

JOSEPH JEROME LE FRANÇAIS LALANDE [de], a most zealous and accomplished astronomer, born at Bourg en Bresse, 11th July, 1732, was the son of Peter Lefrançais, and Marianne Mouchinet, his wife.

His parents were in easy circumstances, and his education being somewhat too indulgent, the natural quickness and impetuosity of his temper was too little restrained. His earliest taste, like that of most other children, seems to have been for romantic tales, and he was fond of making little stories with the materials that he possessed, but their subjects were chiefly religious. He was in the habit of living much with the Jesuits, and he imbibed from them a predilection for the pulpit; at the age of ten he used to amuse himself with making sermons, and preaching them to a select congregation. The comet of 1744, however, with its long tail, took more forcible possession of his imagination, and he watched it with the most unremitting attention. Having been sent to Lyons, to continue his studies under the Jesuits there, he acquired a taste for poetry and eloquence, and was then inclined to devote himself to literature and to the bar; but an eclipse of the sun recalled his attention to astronomy. His parents wished him to follow the profession of a magistrate, and sent him to Paris with that view; but he accidentally lodged in a hotel where Delisle had established an observatory, and this circumstance led him to become acquainted with that professor, and to attend his lectures. These lectures were by no means popular; and the want of a more numerous audience made it easy for the professor to accommodate his instructions to the fixed attention and rapid progress of his new pupil, who became singularly attached to his master, and to all the methods which he employed. Lalande attended, however,

at the same time, the physico-mathematical lectures of Lemonnier, who was more in credit as a teacher, and who also took great pains for his improvement.

In the mean time he had completed his legal studies, and at the age of eighteen he was called to the bar as an advocate. His family was anxious for his return to Bourg; but just at that time Lemonnier obtained leave to nominate him as a substitute for himself on an astronomical mission to Berlin, where he was to make observations on the lunar parallax, corresponding with those which Lacaille was sent to the Cape to obtain. He was favourably received by Maupertuis, who introduced him to Frederic and his court; and was made a Member of the Academy of Sciences at Berlin, when he was about nineteen.

He remained a year in that city, observing at night, and passing his mornings in the study of the integral calculus, under Euler's direction; and his evenings in the society of Voltaire, Maupertuis, D'Argens, and other men of talents. It was not likely that the intercourse with such persons should confirm the principles which he had imbibed from the Jesuits; his moral conduct, however, does not appear to have been influenced by his change of sentiments. After his return to Bourg, he pleaded a few causes to oblige his friends; but the success of his operations at Berlin obtained him speedily a place in the Academy of Sciences at Paris; for, in 1753, before he was twenty-one, he was chosen to fill up a vacancy in the department of astronomy, which had been open for some years. He soon after offended his friend Lemonnier, by rejecting too harshly an unfounded objection of that astronomer to his method of computing the effect of the earth's ellipticity on the lunar parallax, which differed from Euler's formula. Lacaille, who drew up the report of a committee appointed on the occasion, decided in Lalande's favour; but Lemonnier remained dissatisfied, and would not see him for twenty years. He had some similar discussions, at a later period, with Duséjour, who was a little too severe in criticising some of his approximations, as if they had been intended to be rigidly accurate; but their personal friendship remained unaltered.

For more than fifty years he continued to be a constant and voluminous contributor to the *Memoirs* of the Parisian Academy, as well as to other scientific collections. His investigations were always judiciously directed to the advancement of astronomy; but they can scarcely ever be said to have exhibited any marked features of talent, or of address, beyond what might be expected from the industry of a man of good ordinary abilities, confining himself almost entirely to one subject. He was always anxious to call the public attention to astronomy as a science, and to himself as an individual. Thus, on occasion of the transits of Venus in 1761 and 1769, he addressed a circular letter to most of the governments of Europe, on the importance of obtaining a multiplicity of collateral observations, and he received in reply several invitations, from sovereigns whose countries were more favourably situated for the purpose than France, to come and make the observations in person. He thought it, however, unnecessary to leave Paris on the occasion; he contented himself with being the first to announce to the public the result of the most satisfactory comparisons; and his countrymen seemed to give him almost the whole credit of every thing that had been done by others, in conformity with his suggestions. He was much mortified, however, in not receiving from Father Hell an account of the observations made at Wardhus: and he was afterwards greatly inclined to dispute their accuracy, because Hell made the parallax smaller than he did by $\frac{1}{3}$ of a second: while the mean of both results, which is 8, 6″, agrees extremely well with the most modern computations: but, in the end, he did justice to the importance of Hell's observations.

He was constantly in the habit of passing a few months every year with his family in the country, and he occasionally amused himself, in the course of these visits, with mineralogical excursions, and with chemical studies. He delivered, about the year 1758, an oration, before a public assembly at Lyons, on the advantage of monarchy above every other form of government; he even adhered to a similar opinion, and expressed it openly, in times when nothing but his celebrity, as a man devoted exclusively to science, could have made it safe for him to declare it.

After having published the Astronomical Tables of Halley, he felt the necessity of a new collection, and determined to begin with those of Mercury, which he found the most imperfect. He pursued, for this purpose, a regular course of observations at the Palais Royal, where he used to go before sunrise, in the winter mornings, to see the planet in the twilight. Having occasion to refer to the observations recorded by Ptolemy, he found it necessary to refresh his acquaintance with the Greek language, which he had in some measure neglected. But, with all his labour and diligence, his tables of Mercury exhibited, in 1786, an error of 40 minutes in the time of a transit: the circumstance mortified him extremely; but it led to a revision of the tables, and he afterwards succeeded in making them much more perfect. It must be recollected, that, in the time of Hevelius, a transit was anxiously expected for four whole days before it occurred.

He next undertook to improve the tables of Mars and Venus: his tables of these planets were, on the whole, less accurate than those of Mercury, though more exempt from great occasional errors. He had computed their perturbations in the *Memoirs* of the Academy; but he never thought it worth while to compare his formulas with observation. The irregularities of Jupiter and Saturn were much more discouraging; he was obliged to confine himself, in discussing them, to the most modern observations; and he did not appear sufficiently to appreciate the empirical equations of Lambert, though they greatly diminished the errors of Halley's tables.

When Maraldi had given up the management of the *Connaissance des Tems*, Lalande and Pingré were candidates for the appointment. Lalande succeeded in obtaining it; but he had the modesty to confess that the work would have been more accurately performed by Pingré, if his connexion with the church had not, according to the rules of the Academy, incapacitated him for the situation. He made the work, however, much more popular, as a miscellaneous publication, than Pingré was likely to have done; and he was less prejudiced than Pingré in the choice of his tables. He remained editor of the work from 1760 to 1775; it was conducted by Jeaurat

from 1776 to 1787, and from 1788 to 1793 by Méchain: Lalande then undertook it once more; Méchain being engaged in some measurements with Delambre, the Academy having been abolished, and its members dispersed.

Lalande had been disposed to call in question the assertion of Newton and of Voltaire, that no comet could possibly come into contact with the earth; and he had proved that the effect of perturbations at least rendered their reasonings somewhat inconclusive. A short memoir on the subject, which was to have been read at a public sitting of the Academy, was accidentally omitted, as not very important, from the pressure of other business. This circumstance alarmed the sensibility of the public of Paris, who fancied that Lalande had foretold some dreadful catastrophe, which the Government was afraid to announce; and when the memoir was published, they insisted that its contents had been modified, to lessen the alarm. Duséjour made some objections to the author's reasoning: but the whole affair was soon forgotten.

A memoir on the length of the year was honoured with a prize by the Academy at Copenhagen. Delambre, however, thinks the determination not so good as the earlier one of Lacaille, though much better than Mayer's, which was more commonly adopted. Lalande took great pains also with the subject of the sun's rotation, employing in his computations of the places of the spots an easy approximation, instead of Duséjour's more laborious methods; but being careful to compare with each other the most distant observations of the same spot. From the existence of this rotation he thought it reasonable to infer that the sun had also most probably a progressive motion, which would naturally be produced by any single impulse capable of occasioning a rotation. He had some discussions with Dr. Maskelyne, respecting the mode of computing the equation of time, in which Maskelyne appears to have had the advantage.

In the year 1762, Delisle resigned in his favour the Professorship of Astronomy in the Collége de France, which he kept for nearly 46 years. He allowed the most attentive of his pupils to board with him at a cheap rate, doing his utmost

on all occasions to promote their success in their studies and in life. Thus he brought forwards Méchain and Dagelet, and afterwards his own nephew, who completed, with so much diligence and accuracy, the *Description of the Heavens*, which he had himself projected, and which had been begun by Dagelet before his unfortunate expedition. He was made a Fellow of the Royal Society of London in 1763.

His health was generally good, though his constitution was delicate. He had an attack of jaundice in 1767, which was attributed to intense application ; but he completely recovered from its effects by an attention to diet, and by the use of horse exercise. He then intended to leave all his property to the Academy ; but he afterwards gave up his family estates to his relations, and lived on his appointments only, refraining from all kinds of luxuries, in order to be the more able to do acts of liberality to his friends, whom he always sought to oblige in the most delicate manner, and often without making his services known. He had a pension from Russia in the time of the Empress Catherine ; it was suspended by Paul, but restored in 1805 by Alexander.

He was not particularly successful as an observer, but used to refer to the works of his contemporaries, Bradley and Lacaille, though not exactly according to the expression of one of his biographers, "as Ptolemy had done to those of Hipparchus ;" for Hipparchus must have been dead two centuries before Ptolemy was born. On the occasion of the disappearance of the ring of Saturn in 1774, he went to Béziers, in order to profit by the superior serenity of the air there, the climate of that country being supposed to be the best in France ; but his observations were less valuable than others made at Paris and in London.

In the year 1798, he undertook an astronomical expedition to Gotha. He had once meditated an aerostatical voyage there ; but his companion took care that their dangers should terminate in the Bois de Boulogne. He was received with much interest at Gotha by an assembly of astronomers, that was collected from different parts of Germany. The object of the congress was perhaps not unmixed with personal vanity ;

but it had no political design to promote, unless the general adoption of the new French measures could be considered as a political object. Lalande was by no means a revolutionist: he was sufficiently free from any prejudices of education; but he openly condemned the political opinions of the day; and, in 1792, he even exposed himself to great personal danger, in order to save the life of Dupont de Nemours, after the 10th August; and he was equally useful to some of the clergy, whom he concealed in the buildings of the Observatory at the Collége Mazarin, making them pass for astronomers. He had also the courage to publish accounts of Lavoisier and Bailly a short time after their deaths.

The attentions of the German astronomers gave him sincere pleasure. He was at all times extremely sensible to compliments, and even to flattery, though very regardless of satire. He used to call himself a sponge for praise, and an oil-cloth for censure. He professedly believed himself endowed with all the virtues, modesty not excepted. He was so fond of notoriety, that he once undertook to exhibit the variations of the light of Algol to the public of Paris on the Pont Neuf; but the police interfered, thinking it right to prevent a disorderly assemblage.

Though Lalande can only be classed in the second rank as an inventive astronomer, or a mathematician, he certainly stands in the first as a professor and a popular writer. His methods of calculation have in most instances been already superseded by others more convenient or more exact; those which related to particular phenomena for want of sufficient precision, and those which were more general for want of being readily applicable, without continual repetition, to a sufficient number of concurring observations. It has been observed, that he may perhaps have been often too zealous in the pursuit of his favourite objects; but that, if he had possessed more circumspection, and less vivacity of character, he would have been more exempt from criticism, but he would have rendered less important services to science and to mankind.

His last illness was of a consumptive nature, and he seems to have accelerated its termination by attempting too much to

harden himself. He died 4th April, 1807, nearly 75 years old, and in the perfect possession of his faculties. His last words, when he dismissed his attendants to rest, were, "I have need of nothing more," and in a few minutes he was dead. Had he survived a few hours, he would have received a letter from Dr. Olbers, announcing the discovery of a new planet; for which that distinguished astronomer afterwards received the fourth prize-medal upon the institution founded by Lalande in 1802, for the most important astronomical discovery made in the course of the year.*

* In the original there is given a detailed catalogue of all his publications, more than 200 in number, which it has not been thought necessary to reprint. Of his well known Treatise on Astronomy, Dr. Young says, "this compilation far excelled, in utility, all former works of the kind, and will always be considered as exhibiting the most perfect picture of the science, such as it existed from 1760 to 1790, with all the details of practice and computation. Lemonnier called it, with some truth, the great newspaper of astronomy."—*Note by the Editor.*

No. LXXXIV.

LIFE OF LAMBERT.

JOHN HENRY LAMBERT, a natural and moral philosopher of great talent and originality, born 29th August, 1728, at Mülhausen in Upper Alsace, was the son of a French refugee in a very humble station, and one of a numerous family.

His early studies were only assisted by the instruction he obtained at a small free school in his native town. His father, who was a tailor, could scarcely even afford him leisure from mechanical labour; he was obliged to read and write in the night, and in order to procure candles, he made little drawings for sale, while he was watching the cradle of his infant sisters. Having learned to write a good hand, he obtained some employment as a copying clerk in the chancery of the town, which he gave up, when he was only 15, upon being appointed book-keeper at some ironworks in the neighbourhood. At 17, he became secretary to a Doctor Iselin, who was the editor of a newspaper at Bâle, and who became his firm friend through life. He had now time to render himself familiar with the works of Wolf, Locke, and Malebranche, to which he was in a great measure indebted for the correct logical method that he ever after followed in his researches; having, however, confirmed and improved it by the study of the mathematics, to which he devoted himself with great zeal, and which, after all, constitutes the best practical school of genuine logic.

In 1748, he removed to Coire, having been recommended by Iselin, as private tutor to the family of the President Count Peter de Salis, whom he undertook to instruct in history and religion, as well as in languages and science. The library of his patron was extensive; he profited by it in all its departments; and his residence at the house of an accomplished

statesman, frequented, as it was, by the best informed persons of different countries and with different pursuits, could not but greatly contribute to the extension of his knowledge, and the improvement of his taste. He even amused himself with some poetical exercises in various languages, which must, at least, have been of advantage to his style in prose. He felt the importance of his literary and scientific pursuits to himself and to the world, and in 1752 he determined to keep a journal of all his studies, which he continued throughout his life; he began to publish a variety of fugitive pieces, on different subjects, in the newspapers and in other periodical works of the day, some of which attracted the notice of his learned countrymen; and, in 1754, he was made a member of the Physico-Medical Society, then lately established at Bâle, to the *Transactions* of which he contributed many interesting papers. In 1756 he went to Göttingen with two of his pupils, and in 1757 to Utrecht. The next year the party returned to Coire, by way of Paris, Marseilles, and Turin. At Paris he paid a visit to D'Alembert, who does not appear at that time to have appreciated his merit very highly, though he afterwards rendered him some services with the King of Prussia; but he became more intimately acquainted with Messier the astronomer.

In 1759 he quitted the family of the Count de Salis, and went to settle at Augsburg, having a small salary as a member of the Electoral Academy of Bavaria. From 1761 to 1763 he was again at Coire and in its neighbourhood, being employed in fixing the boundaries between the country of the Grisons and the Milanese territory. Towards the end of 1763, having had some disputes with the Bavarian academicians, he went to Leipzig, and the next year to Berlin, where he was made a member of the Royal Academy of Sciences, and where he continued to reside during the remainder of his life, receiving many marks of favour from the discriminating liberality of Frederic: thus, in the year 1770, he was made superior counsellor of the Board of Works, with an additional salary. He contributed a number of valuable memoirs to the collection of the Academy; and in 1774 he undertook the direction of the *Astronomical*

Almanac, for which he was admirably qualified. He was also a constant writer in the journal published by Nicolai, under the title of the *Universal German Library;* and he kept up a very extensive correspondence on various subjects of literature and science.

He was regularly in the habit of writing or reading from five in the morning till twelve, and again from two till midnight; a degree of application unquestionably far beyond that which would have been best calculated for producing the maximum of valuable effect.—Perhaps, if he was paid for writing by the ream, he may have earned as much from the booksellers as he would have done by a more judicious economy of his powers; but a nervous system, attenuated by the daily study of 17 hours, could never have been capable of being employed in any very elevated flights of genius, or in the invention of any sublime or exquisite novelties either in science or in literature: and it is only wonderful that he did any thing so well, as almost to form an exception to this general remark. He was indeed supposed to have injured his health by continued application, and he died consumptive at 49, the 25th September, 1777. He had never been married. His person was of the middle size, with an interesting and expressive countenance; he was animated and lively in conversation, and liked discussion, but not disputation. He had no literary quarrels; and his criticisms were not offensive, even when they ceased to be flattering. His morals were strictly correct, but his manners were not altogether in unison with those of the society to which his talents had elevated him: he is said to have been timid, awkward, slovenly, and fond of low company; but upright, patient, unostentatious, and compassionate; essentially modest, but as ready to assert his own merits as to admit his defects. He had a happy facility in managing the instruments of computation, especially in the arrangement of converging series; and he had a peculiar talent for expressing the results of observation by an analytical formula; having first thrown them into the form of a geometrical diagram to assist his invention; a process which he employed with regard to the probabilities of life in London,

and to the inequalities of Jupiter and Saturn. In short, after Euler, Lagrange, D'Alembert, and Daniel Bernoulli, there are few mathematicians and natural philosophers of any age who can be put in competition with him, and still fewer who benefited the public by so many diversified labours.*

* In the original is given a lengthened catalogue of his works, which it has not been thought necessary to reprint.—*Note by the Editor.*

No. LXXXV.

LIFE OF MASKELYNE.

NEVIL MASKELYNE, a most industrious and accurate astronomer, born in London, 6th October, 1732, was the son of Edmund Maskelyne, Esq., a gentleman of respectable family of Purton in Wiltshire.

He was sent at the age of nine to Westminster School, and continued to apply with diligence to the usual pursuits of that place, until the occurrence of the great solar eclipse of 1748, which made a strong impression on his mind, and which was the immediate cause of his directing his attention to astronomy, and of his beginning the study of the mathematics with great ardour, as subservient to that of astronomy. It is remarkable that the same eclipse is said to have made an astronomer of Lalande, who was only three months older than Maskelyne. He soon after entered as a member of Catherine Hall, Cambridge, but shortly removed to Trinity. He took a degree as Bachelor of Arts with great credit in 1754, and proceeded regularly afterwards through the succeeding stages of academical rank in divinity. He was ordained in 1755 to a curacy at Barnet, and the next year obtained a fellowship at Trinity. In 1758 he was elected a fellow of the Royal Society, having previously become intimate with Dr. Bradley, and having determined to make astronomy the principal pursuit of his life, feeling its perfect compatibility with an enlightened devotion to the duties of his own profession.

In 1761 he was engaged by the Royal Society to undertake a voyage to St. Helena, in order to observe the transit of Venus. He remained ten months in the island, but the weather prevented his observing the transit to advantage, and the faulty attachment of the plumb-line of his quadrant, which was of the construction then usually employed, rendered his

observations on the stars less conclusive with respect to annual parallax than he had expected. His voyage was, however, of great use to navigation, by promoting the introduction of lunar observations for ascertaining the longitude; and he taught the officers of the ship which conveyed him the proper use of the instruments, and the mode of making the computations.

He performed a second voyage, in 1763, to the island of Barbadoes, in order to determine the rates of Harrison's watches, and to make experiments with Irwin's marine chair, on board of the Princess Louisa, Admiral Tyrrel, acting at the same time as chaplain to the ship. The chair he found of very little use for observing the eclipses of Jupiter's satellites, and the maker of the chronometers was not satisfied with his report of their performance; fancying that he was too partial to the exclusive employment of lunar observations for determining the longitude. The liberality of the British Government, however, bestowed on Harrison the whole reward that he claimed; and Maskelyne, having been appointed to the situation of Astronomer Royal, and having thus become a member of the Board of Longitude, was extremely active in obtaining a few thousand pounds for the family of Professor Mayer, who had computed lunar tables, and a compliment of 300*l.* only for Euler, whose theorems had been employed in the investigation.

The merits of Mayer's tables having been fully established, the Board of Longitude was induced to promote their application to practical purposes, by the annual publication of the *Nautical Almanac*, which was arranged and conducted entirely under Maskelyne's direction for the remainder of his life. He was also actively employed, without any other motive than the love of science and of his country, in almost every decision which was required of the Board of Longitude; and he had to give his opinion of the merits of an infinite number of fruitless projects which were continually submitted to his judgment. He must of course have made many enemies among the weak and illiberal; but the universal impartiality, and the general accuracy of his determinations, were acknowledged by all candid persons; and it must be admitted that the longitudinary speculators of Great Britain do in general submit to discou-

raging remarks from persons in authority with wonderful fortitude, and with great personal civility.

During the forty-seven years that he held the situation of Astronomer Royal, he acquired the respect of all Europe by the diligence and accuracy of his observations, which he never neglected to conduct in person whenever it was in his power, and he required only one assistant. The French had a handsome building to amuse the public by its exterior magnificence, but the establishment of the observers was never arranged in so methodical a manner as that of the English National Observatory, and the fruits of their labours were never systematically made public; the attempt, which was once made by Lemonnier, in his *Histoire Céleste*, having been interrupted and discontinued. Dr. Maskelyne, on the other hand, obtained leave from the British Government to have his observations printed at the public expense, under the direction of the Royal Society, who are the legal visitors of the Observatory, appointed by the King's sign manual. The early observations of Flamsteed and Bradley were considered as private property: Flamsteed published his own, and Bradley's were very liberally bought of his family, and afterwards printed by the University of Oxford, who are still as liberal in bestowing them where they are likely to be employed for the benefit of science. Flamsteed was the Astronomer Royal from 1690 to 1720; then Halley to 1750; Bradley to 1762; and Bliss to 1765, when Maskelyne was appointed. He took his doctor's degree in 1777.

He made several improvements in the arrangement and employment of the instruments, particularly by enlarging the slits through which the light was admitted; by making the eye-glass of his transit telescope moveable to the place of each of the wires of the micrometer; and above all, by marking the time to tenths of a second, which had never been attempted before; but which he found it practicable to effect with surprising accuracy, as the comparison of the observations at the different wires sufficiently demonstrated.

The object of his expedition to Shehallien is well known. Bouguer had made an unsuccessful attempt to measure the attraction of a mountain in South America, and had been

obliged to conclude that the mountain was hollow, in consequence of the eruption of a volcano, the attraction being too little sensible. Dr. Maskelyne's results, on the other hand, as computed by Dr. Hutton, made the mountain more dense than could well have been expected; but those who are acquainted with the difficulty of executing astronomical measurements without an error of a single second of space, will be ready to allow that the deviation of 5″ or 6″, attributed to the effect of the mountain, is liable to a much greater proportional uncertainty than the results obtained by Mr. Cavendish with the apparatus invented by Mr. Michell. (See Life of CAVENDISH, No. LXVII.) The geodetical operations which were soon afterwards performed with his concurrence and assistance, for determining the relative situations of Greenwich and Paris, were equally creditable to the English artists who constructed the instruments, and to the astronomers and geographers who made the observations with them; and they excelled, even by the confession of their rivals, everything that had ever been effected in former measurements of the same kind.

As no man had done more for practical astronomy than Dr. Maskelyne, so there was none whose merits were more justly appreciated. He made every astronomer his friend, as well by his personal kindness, as by his professional labours; and he obtained the rare distinction of being made one of the eight foreign associates of the Parisian Academy of Sciences. His example and encouragement contributed to the establishment of several private observatories, which must always be, if not immediately, at least remotely, beneficial to astronomy, as tending to promote the improvement of instruments, and of the methods of employing them.

He was modest, and somewhat timid, in receiving the visits of strangers; but his usual conversation was cheerful, and often playful, with a fondness for point and for classical allusion. He inherited a good paternal property, and he obtained considerable preferment from his college; he also married, somewhat late in life, the sister and co-heiress of Lady Booth of Northamptonshire. His sister was the wife of Robert Lord Clive, and the mother of the present Earl of Powis. He died the 9th

of February, 1811, in his 79th year, leaving a widow and an only daughter.

1. Dr. Maskelyne's first communication to the Royal Society is *A Proposal for discovering the Annual Parallax of Sirius. Phil. Trans.* LI. 1760, p. 889. It is founded on Lacaille's observations, made at the Cape of Good Hope, which appeared to indicate a maximum amounting to 8″. 2. *A Theorem for Spherical Aberration*, LII. 1761. p. 17. Dated from the Prince Henry, St. Helen's Roads: the calculation is adapted to the object-glasses of achromatic telescopes. 3. The next article, p. 21, is a letter from Lacaille, recommending him to make observations at St. Helena on the lunar parallax, and to remain some time in the island for that purpose; promising, on his own part, to make corresponding observations. It is followed by a *Letter* from Maskelyne, proposing some *additional joint observations*. 4. *Observation of the Transit of* 1761, p. 196. The sun was lower than had been expected, and the instant of contact uncertain, from a tremulous motion in the apparent discs. 5. *Observations on a Clock of Shelton*, 1762, p. 434. Giving the proportion of .99754 to 1 for the comparative force of gravity at Greenwich and at St. Helena. 6. *A Letter on the mode of observing and computing Lunar Distances*, p. 558. Dated from St. Helena: the first demonstration of the practicability and utility of the method. He found the error of observation not to exceed half a degree of longitude, an error which was very strangely suffered to remain as a fair allowance for the uncertainty of observation, in the Acts for encouraging the perfection of the lunar tables, only very lately repealed. 7. *On the Tides at St. Helena*, p. 586. Observations made in a harbour, for about two months. 8. *Note to Lalande*, p. 607. On Lunar Distances and Occultations. 9. *Rules for correcting Lunar Distances. Phil. Trans.* 1764, p. 263. A demonstration of the rules before published in the *Transactions* and in the *British Mariner's Guide*. 10. *Remarks on the Equation of Time*, p. 336. Correcting a mistake of Lacaille, and an inadvertence of Lalande, and giving a formula, which, though not geometrically perfect, is abundantly accurate for all practical purposes. 11. *Astronomical Observations made at St. Helena*, p. 348. The obser-

vations for determining the lunar parallax were too few to afford a satisfactory result. The author suggests, that the figure of the earth might be ascertained by repeated and comparative observations of the apparent distance of the moon from neighbouring stars. 12. *Observations made at Barbadoes*, p. 189. Especially on Jupiter's satellites. 13. *Introduction to two Papers of Mr. Smeaton.* LVIII. 1768, p. 154. The one on the menstrual parallax, the other on observing stars out of the meridian. 14. *Introduction to the Observations of Mason and Dixon*, p. 270. 15. *Conclusion respecting the Length of a Degree*, p. 323, 325. Mr. Charles Mason had been sent with Mr. Dixon to observe the transit of 1761, at Bencoolen, but their voyage was interrupted by accidental circumstances, and they made their observations at the Cape of Good Hope, with tolerable success. They then proceeded to join Maskelyne at St. Helena, and to assist in his operations there. They were afterwards engaged by Lord Baltimore and Mr. Penn, to determine the boundaries between Maryland and Pennsylvania; and having completed their survey, they suggested to the Council of the Royal Society the eligibility of measuring a degree in the country bordering on the Delaware and Chesapeake. Their proposals were readily accepted, and the results of their measurement are here recorded. Dr. Maskelyne afterwards employed Mason, in his operations on Shehallien, and in computing Bradley's observations, and in improving Mayer's tables, by a comparison with them; but he was so fearful of admitting any empirical corrections, not founded on the most general principles, that he would not allow some of the equations, discovered by Mason, to be introduced into the computation of the *Nautical Almanac*, until Laplace had proved their dependence on the theory of gravity. Lalande tells us, that Mason was dissatisfied because he did not receive a public reward for the success of his labours; but he was, in fact, little more than the agent of Maskelyne, and of the Board of Longitude; and he was fairly repaid for the time and labour which his computations had required. Delambre says, that he died in Pennsylvania in 1787. Dixon is said to have been born in a coal mine; and to have died at Durham in 1777. 15. *Post-*

script respecting French and English Measures, p. 325. The result of this comparison agrees admirably well with the later measurement of Pictet, Prony, and Captain Kater. 16. *Observation of the Transit of* 1769, *made at the Royal Observatory*, p. 355. 17. *Eclipses and Occultations*, 1769, p. 399, chiefly for the longitude of Glasgow. 18. *On the use of Dollond's Micrometer*, 1771, p. 536. On the application of the divided object-glass micrometer to determining differences of right ascension and of declination, especially in the case of transits. A part of the instructions sent with the observers to the South Seas. 19. *On the Adjustment of Hadley's Quadrant*, 1772, p. 99. Especially for the back observation; and to insure the parallelism of the glasses. 20. *Deluc's Rule for measuring Heights*, 1774, p. 158. Adapted to English measures, and rendered somewhat more convenient. 21. *Observations at Greenwich and in America compared*, p. 184, 190. 22. *Proposal for measuring the Attraction of a Hill*, 1775, p. 495. Read in 1772. 23. *Observations made on Shehallien*, p. 500. A paper which obtained its author the honour of a Copleian medal. Mason had been sent to examine the hills of Scotland, and had recommended Shehallien; the funds were supplied by the remainder of the royal grant for observing the transit of Venus. Mr. Reuben Burrow and Mr. Menzies were principally employed in assisting the Astronomer Royal in his observations and surveys; and Dr. Hutton afterwards made the necessary computations for determining the attraction of the mountain. 24. *Description of a Prismatic Micrometer*, 1777, p. 799. Consisting of one or more prisms sliding in the axis of the telescope; and resembling in its operation that of Rochon, which has in great measure superseded it. 25. *On the Longitude of Cork*, 1779, p. 179. Observations for correcting the computed times of the eclipses of Jupiter's satellites. 26. *On the Comet expected in* 1789, *Ph. Tr.* 1786, p. 426. Supposing those of 1532 and 1661 to be the same. (See MÉCHAIN.)* 27. *On the Latitude and Longitude of Greenwich*, 1787, p. 151. With Cassini's memoir on its uncertainty, which he states as amounting to 11″ in longitude, and 15″ in latitude. Dr. Maskelyne, however, shows that it is

* See *infra*, p. 623, note.

confined within much narrower limits, though he approves of the object of the memoir in promoting a survey. 28. *On a Difficulty in the Theory of Vision*, 1789, p. 256. This paper sufficiently proves, that Euler was mistaken in thinking the eye achromatic; and that any appearance of colour which it could produce, according to the common laws of refraction, would be imperceptible in ordinary circumstances. But that there are circumstances, under which such appearances may be observed, has been more lately shown by Dr. Young and Dr. Wollaston. 29. *Account of an Appearance of Light on the Dark Part of the Moon*, 1793, p. 429. Seen by Mr. Wilkins, and by a servant of Sir George Booth, and supposed to have arisen from a volcano. 30. *Observations of the Comet of* 1793, *Ph. Tr.* 1794, p. 55. Discovered by the Rev. E. Gregory, of Langar, in Nottinghamshire.

31. The earliest of Maskelyne's separate publications was his *British Mariner's Guide*, 4. Lond. 1763. A small volume, which has become scarce, having been superseded by later works.

32. *The Nautical Almanac and Astronomical Ephemeris* for 1767 appeared in 1766; and the publication has been regularly continued upon the same plan to the present time, by the computers and comparers whom Dr. Maskelyne had trained by his instruction and example. His successor in the Observatory, though admirably qualified to equal, and perhaps to excel him in the practical department, had it not in his power to devote so much of his attention to the publication, as Dr. Maskelyne's paternal affection for a child of his own had induced him to bestow on it: and the Board of Longitude was very liberally furnished by the present ministry, with the means of obtaining some further assistance to supply his place.

33. *Tables requisite to be used with the Nautical Almanac*, 8. Lond. 1766, 1783, 1802. Now partly superseded by Professor Lax's new edition.

34. The volume of *Selections*, from the additions that have been occasionally made to the *Nautical Almanac*, 8. Lond. 1812; contains several papers of Dr. Maskelyne. For example, *Instructions relating to the Transit of Venus in* 1769, N.A. 1769 *Elements of Lunar Tables*, and *Remarks on Hadley's Quadrant*,

N. A. 1774. *Advertisement of the Comet expected in* 1788, N. A. 1791. *On the Disappearance of Saturn's Ring in* 1780, N. A. 1791.

35. The *Astronomical Observations made at Greenwich*, from 1765 to 1811, were published annually in folio; making three volumes and part of a fourth, Lond. 1774.... They are allowed to constitute the most perfect body of astronomy in detail that was ever presented to the public. The first volume contains a variety of useful tables, accompanying the observations for 1772; and principally serving for the correction of the places of the stars, and for facilitating the solution of other astronomical problems. Many of them have been reprinted in Vince's *Astronomy*; but, in some cases, without the necessary explanations.

No. LXXXVI.

LIFE OF ATWOOD.

George Atwood, an author celebrated for the accuracy of his mathematical and mechanical investigations, and considered as particularly happy in the clearness of his explanations, and the elegance of his experimental illustrations, was born in the early part of the year 1746. He was educated at Westminster School, where he was admitted in 1759. Six years afterwards he was elected *off* to Trinity College, Cambridge. He took his degree of Bachelor of Arts in 1769, with the rank of third wrangler, Dr. Parkinson, of Christ's College, being senior of the year. This distinction was amply sufficient to give him a claim to further advancement in his own College, on the list of which he stood foremost of his contemporaries; and, in due time, he obtained a fellowship, and was afterwards one of the tutors of the College. He became Master of Arts in 1772; and, in 1776, was elected a Fellow of the Royal Society of London.

The higher branches of the Mathematics had, at this period, been making some important advances at Cambridge, under the auspices of Dr. Waring, and many of the younger members of the University became diligent labourers in this extensive field. Mr. Atwood chose, for his peculiar department, the illustration of mechanical and experimental philosophy, by elementary investigations and ocular demonstrations of their fundamental truths. He delivered, for several successive years, a course of lectures in the Observatory of Trinity College, which were very generally attended, and greatly admired. In the year 1784, some circumstances occurred which made it desirable for him to discontinue his residence at Cambridge; and soon afterwards Mr. Pitt, who had become acquainted with his merits by attending his lectures, bestowed

on him a patent office, which required but little of his attendance, in order to have a claim on the employment of his mathematical abilities in a variety of financial calculations, to which he continued to devote a considerable portion of his time and attention throughout the remainder of his life.*

The following, we believe, is a correct list of Mr. Atwood's publications:

1. *A Description of Experiments to illustrate a Course of Lectures.* 8vo. About 1775 or 1776.

2. This work was reprinted with additions, under the title of *An Analysis of a Course of Lectures on the Principles of Natural Philosophy.* 8vo. Camb. 1784.

3. *A General Theory for the Mensuration of the Angle subtended by two objects, of which one is observed by Rays after two Reflections from plane Surfaces, and the other by Rays coming directly to the Spectator's Eye.* Phil. Trans. 1781, p. 395.

4. *A Treatise on the Rectilinear Motion and Rotation of Bodies, with a Description of Original Experiments relative to the Subject.* 8vo. Cambr. 1784.

5. *Investigations founded on the Theory of Motion, for determining the Times of Vibration of Watch Balances.* Phil. Trans. 1794, p. 119.

6. *The Construction and Analysis of Geometrical Propositions, determining the positions assumed by homogeneal bodies, which float freely, and at rest on a fluid's surface; also Determining the Stability of Ships, and of other Floating Bodies.* Phil. Trans. 1796, p. 46.

7. *A Disquisition on the Stability of Ships.* Phil. Trans. 1798, p. 201.

8. *A Review of the Statutes and Ordinances of Assize, which have been established in England from the 4th year of King John,* 1202, *to the 37th of his present Majesty.* 4to. Lond., 1801.

9. *A Dissertation on the Construction and Properties of Arches.* 4to. Lond. 1801.

* See 'Literary Memoirs of Living Authors,' 2 vols. 8vo., Lond. 1798; 'Morning Herald,' 17th July, 1807; 'Nichols's Literary Anecdotes of the Eighteenth Century,' vol. viii., 8vo., Lond. 1814.

10. *A Supplement to a Tract entitled a Treatise on the Construction and Properties of Arches, published in the year* 1801 ; *and containing Propositions for determining the weights of the several sections which constitute an arch, inferred from the angles. Also containing a Demonstration of the angles of the several sections, when they are inferred from the weights thereof. To which is added, a Description of original experiments to verify and illustrate the principles in this treatise. With occasional remarks on the construction of an iron bridge of one arch, proposed to be erected over the river Thames at London. Part* II. *By the author of the first part.* 4to. Lond. 1804. Dated 24th Nov. 1803.

11. *A Treatise on Optics* is mentioned by Nichols as having been partly printed by Bowyer in 1776, but never completed.

It may be very truly asserted, that several of these works of Mr. Atwood have materially contributed to the progress of science, by multiplying the modes of illustration, which experimental exhibitions afford for the assistance of the instructor; at the same time, they can scarcely be said to have extended very considerably the bounds of human knowledge, or to demonstrate that their author was possessed of any extraordinary talent or energy of mind in overcoming great difficulties, or in inventing new methods of reasoning. The *Analysis of a Course of Lectures* has been little read: and it bears evident marks of having been composed before Mr. Atwood had acquired a habit of accurate reasoning on Physical subjects. In the first page, for example, the forces of cohesion and gravitation are completely confounded ; and in the third we find the idea of perfect spheres touching each other in a greater or less number of points, notwithstanding the appearance of precision which the author attempts to maintain in his language.

The object of the paper on *Reflection* is, to illustrate and improve the construction of Hadley's quadrant ; and Mr. Atwood proposes, for some particular purposes in practical Astronomy, two new arrangements of the speculums, by which the rays are caused to move in different planes, and which he considers as affording greater accuracy for the measurement of

small angles than the common form of the instrument, although not of general utility, nor very easily adjusted for observation.

The treatise on *Rectilinear Motion and Rotation* exhibits a good compendium of the elementary doctrines of mathematical Mechanics; but it shows a great deficiency in the knowledge of the higher refinements which had been introduced into that science by Daniel Bernoulli, and Euler, and Lagrange. The properties of simply accelerated and retarded motion are first discussed, and the phenomena of penetration experimentally examined; the laws of varying forces are then investigated, and the properties of the pendulum demonstrated; the vibrations of an elastic chord are calculated, "considering the whole mass to be concentrated in the middle point," as an approximation; and then, instead of imitating and simplifying the elegant but complicated demonstrations of the Continental mathematicians, the author most erroneously repeats, in the words of Dr. Smith, the exploded doctrine, that "the string, during any instant of its vibration, will coincide with the harmonic curve." The subject of a resistance, varying as the square of the velocity, is next examined; and some useful experiments on the descent of bodies in water are stated in confirmation of the theory. On this occasion, the author observes, with regard to the formation of the different strata of the earth, that bodies disposed to break into large masses, though specifically lighter, may easily have descended more rapidly through a fluid, than denser but more brittle bodies, so that the natural order of densities may thus have become inverted. He next examines the theory of rotation, and relates some very interesting experiments on rectilinear and rotatory motions; and he shows that Emerson and Desaguliers were totally mistaken in asserting "that the momentum produced is always equal to the momentum which produces it." The last section of the work, which is devoted to the subject of free rotation, is the most elaborate of the whole; but it exhibits no material extension of the earlier investigations of the Bernoullis and Professor Vince; nor does it contain the important proposition of Segner, relating to the existence of three axes of permanent rotation, at right angles to each other, in every body, however irregular.

Notwithstanding these partial objections, the work may still, in many respects, be considered as classical. The paper on *Watch-balances* is principally intended to show the advantages which may be obtained, in Mr. Mudge's construction, from the effect of subsidiary springs in rendering the vibrations isochronous, their actions being limited to certain portions of the arc of motion. If the author has here again omitted to follow the Continental mathematicians in some of their refinements of calculation, it must be confessed that his view of the subject has, in this instance, not only the advantage of simplicity, but also that of a nearer approach to the true practical state of the question, than is to be found in the more complicated determinations which had been the result of the labours of some of his predecessors.

But, whatever may be the merits of these investigations, they appear to be far exceeded in importance by the papers on *Ships*, the first of which obtained for its author the honour of a Copleian Medal. Its principal object is to show how much the stability of a ship will commonly vary, when her situation, with respect to the horizon, is materially altered; and how far the assumptions of theoretical writers, respecting many others of the forces concerned in Naval Architecture, will generally differ from the true state of these forces when they actually occur in Seamanship. In the second part of the investigation, some errors of Bouguer and of Clairbois are pointed out, and the theoretical principles of stability are exemplified by a detailed calculation, adapted to the form and dimensions of a particular vessel, built for the service of the East India Company.

The latter years of our author's life do not appear to have been productive of any material advantage to science. His application to his accustomed pursuits was unremitting; but his health was gradually declining. He had no amusement, except such as was afforded by the continued exercise of his mind, with a change of the object only; the laborious game of chess occupying, under the name of a recreation, the hours which he could spare from more productive exertions. He became paralytic some time before his death; and though he partially recovered his health, he did not live to complete his 62nd year.

His researches concerning the history of the *Assize of Bread* must have required the employment of considerable diligence, and some judgment, in the discovery and selection of materials; although certainly the subject was not chosen for the purpose of affording a display of talent. His opinion respecting the operation of the assize, as favourable to the community, may by some be thought to be justified by the want of success which has hitherto attended the experiment on its suspension; but the advocates of that measure would certainly not admit the trial of a year to be sufficient for appreciating its utility.

The title-pages of the works on *Arches* explain the occasion on which they were brought forward, and at the same time exhibit a specimen of the want of order and precision which seems to have begun to prevail in the author's faculties: and the works themselves betray a neglect of the fundamental principles of Mechanics, which is inconceivable in a person who had once reasoned with considerable accuracy on mathematical subjects. An anonymous critic, who is supposed to have been the late Professor Robison (*British Critic*, vol. XXI. Jan. 1804), very decidedly, and at the same time very respectfully, asserted Mr. Atwood's error in maintaining, that there was no manner of necessity for the condition, that the general curve of equilibrium of an arch should pass through some part of every one of the joints by which it is divided: and in fact we may very easily be convinced of the truth of this principle, if we reflect that the curve of equilibrium is the true representative of the direction of all the forces acting upon each of the blocks; and that if the whole pressure be anywhere directed to a point situated beyond the limit of the joint, there can be nothing whatever to prevent the rotation of the block on the end of the joint as a centre, until some new position of the block shall have altered the direction of the forces, or until the whole fabric be destroyed. The critic has also very truly remarked, that the effects of friction have never been sufficiently considered in such arrangements: but a later author has removed a considerable part of this difficulty in an anonymous publication, by showing, that no other condition is required for determining these effects, than that every joint should be per-

pendicular to the direction of the curve of equilibrium, either accurately, or within the limit of a certain angle, which is constant for every substance of the same kind, and which he has termed the angle of repose (see *supra*, p. 182).

In the appointment which enabled Mr. Atwood to devote a considerable part of his life to scientific researches, he appears to have had no successor. It was held, and perhaps wisely, that such sinecures have regularly become, in process of time, mere instruments of party interest, instead of being bestowed as encouragements to merit; and it seems to be the invariable maxim of the British Government, that talents deserve no protection, unless they are immediately employed in the service of the Church or of the State; that ornamental accomplishments repay their possessor by the splendour which they confer on his personal existence; but that, in a commercial country, the actual utility of mental as well as of corporal powers must be measured by their effects; and that these effects must be of a negociable kind, in order to have a claim to reward Other Nations, and other Sovereigns, have often thought and acted differently, and they have, perhaps, obtained a forced growth of Science or of Literature, which has contributed, in some degree, to the embellishment of their age; but where the native forest tree acquires so often a form at once beautiful and magnificent, though exposed to all the storms of the seasons, there is little reason to lament the want of the shelter of the Plantation, or of the artificial warmth of the Conservatory.

The lives of the following men of science in the Supplement to the 'Encyclopædia Britannica' were also written by Dr. Young, but did not appear to the Editor of sufficient importance to be reprinted:—Giambattista Beccaria, Black, Boulton, Bramah, Brisson, Camus, Cavallo, Duhamel, F. Fontana, G. Fontana, J. R. Forster, J. G. A. Forster, Frisi, Guyton de Morveau, Lemonnier, De Luc, Méchain, Messier, Pallas, and Rush.—*Note by the Editor.*

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